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NOTES AND COMMENTS.

Zoological Nomenclature.

IN our next Note, we attempt to express, so far as we can at all agree with them, the views of the younger and, perhaps, extremer advocates of reform in nomenclature. In these columns, from time to time, we have urged the necessity of immediate and concerted action, and it was with a lively gratitude that we regarded the action of Mr. Sclater in raising the question at a meeting of the Zoological Society. The attitudes of the extreme conservatives and of the keen reformers are to our mind alike inevitable and undesirable. Those who have been accustomed through years of laborious work to particular names not unnaturally magnify the evil of change. Those who have more recently entered upon systematic work, and who find that a great part of their time is absorbed, not by science, but by hunting through a confused synonymy that varies practically with every country and with every worker, have seized upon the principle of priority as a court from which there can be no appeal. The worst of it is that many of these workers, seduced by the idea of finality, have made the hunting of original names an end in itself, and have believed themselves to be advancing science when they were only adding to its confusion.

We hold that the disputants of either side were wrong in trying to appeal to principle. There is no principle involved in the matter; only the most vulgar expediency. Numbers would suit the purpose of designating organic conceptions as well as names, and for purposes of identification one name is in itself as good as another. If a man's fame only rests on the possibility of attaching his name, with or without an intervening comma, to the particular designation of so many species, we cannot do more than shed a passing tear upon him if the general advantage of science should blot out for ever his poor name and personality. In our next paragraph we discuss a proposal based on no principle or justice or right, but one that would autocratically rid us of confusion, and put an end to the constant changes of zealous pedants.

ZOOLOGICAL NOMENCLATURE—A PROPOSAL.

THE discussion on zoological nomenclature, which was held, as announced in our last number, by the Zoological Society of London on March 3, was introduced to a crowded meeting by Mr. P. L. Sclater, F.R.S., in a concise and careful paper, and the points to which he drew attention were warmly debated beyond the usual hour. The discussion dealt with certain differences between the rules drawn up by the German Zoological Society for the guidance of the compilers of the Synopsis of the Animal Kingdom ("Das Tierreich") which that Society is preparing, and the rules known as the Stricklandian Code, which for many years governed, or were supposed to govern, the usage of British naturalists. The discussion turned chiefly upon the following questions:—First, may the same generic names ever be used for both animals and plants? Secondly, may the same term be used for the generic and trivial name of a species, as in the well-known instance of *Scomber scomber*? Thirdly, are we to adopt as our starting-point the tenth edition of Linné's *Systema Naturæ* in preference to the twelfth edition? These questions are answered in the affirmative by the German code, and in the negative by the original Stricklandian. We do not propose to discuss them here: it is natural that there should still be found, especially among the older zoologists of this country, many to support the old-established British practices; in this, as in all other matters of nomenclature, convenience, not principle, is concerned, and it cannot be gainsaid that the general usage of zoologists, at all events in other parts of the world, becomes daily more and more in harmony with the rules adopted by the German Society.

Were we again to open our pages to the discussion of this thorny subject, we should probably prefer, as did many of those who spoke at the Zoological Society's meeting, to discuss points that appear of more vital importance; but after listening to the various ingenious arguments, and to the animated rhetoric, punctuated by shouts of applause, that were poured forth the other evening, we felt more inclined than ever to doubt the value of these discussions. There are, it appears to us, fundamental defects that so far have pervaded all of them. A casual glance at the list of modern codes of nomenclature exhibited by Mr. Sclater was enough to show how very limited has been the authority of those bodies that have, from time to time, ventured to suggest laws for the zoological world. Either it is a committee of a section of the British Association, or it is the Zoological Society of France, or of Germany; or, again, at one moment we find the ornithologists meeting in conclave, at another, the palæontologists, at yet another, the neontologists; even when we see a code drawn up and passed by two International Congresses of zoology, we must not, as the President pointed out, flatter ourselves that more than a very few of the actual workers have assented, or have even been consulted. Consequently, the best of the codes that has yet been proposed (and

which that be, each reader must decide for himself) has lacked the authority and the sanction that alone can make it of value. For we must insist upon this point, if upon no other, that it is not the wording of any particular law that is of consequence, but the power of enforcing it. We venture to say that to the very best code that could possibly be drawn up each individual zoologist would remain a recalcitrant, were it only in so trivial a point as the insertion of a comma or the use of a capital letter.

If it be true that we come to some such *impasse* in whatever direction we proceed, it is worth considering whether we cannot follow some course more productive of finality than is this perpetual codifying of our whims and fancies. And here we would take up and push to their logical conclusion the suggestions that were thrown out at the meeting by Mr. H. J. Elwes and the President. It is not enough to imitate Mr. Elwes, and to follow the last monograph or the last catalogue of some great museum; for other monographers will arise, and rival museums will publish rival catalogues, each with its own system of nomenclature. Nor is it of much use to follow those British ornithologists of whom the President told us, who some years ago made a vow to adopt such and such fixed names for all the British birds; for the science of zoology is not confined to these islands, and those who withdraw from the main stream of progress will either find themselves left high and dry, or be forced to rejoin it as laggards and out-of-date. But the course that might be pursued is suggested to us by this very enterprise of the German Zoological Society. Let us suppose that, instead of shrinking from the magnitude of the undertaking, instead of insinuating its impossibility, and instead of drawing their purse-strings tighter, the zoologists of the world were to give a mandate to the German Zoological Society to proceed with the work, and were to assist them generously by every means in their power, then we should have a complete set of names for all living species of animals. This, it is true, would not be enough. To draw up such a correct list of names without consulting the palæontologists is impossible, and, even were such a list drawn up, it would, for the purpose we now intend, be valueless. But let us further suppose that some body, such as the German or the English Zoological Society, could be found to draw up a list of all animal species, fossil as well as recent, then it would at all events be perfectly possible for the zoologists of the world to accept that list, and to say, "Whether these names be right or wrong according to this or that code of nomenclature, we do not know and we do not care; but we bind ourselves to accept them in their entirety, and we hereby declare that the date when this list was closed for the press shall henceforward be the date adopted as the starting-point for our nomenclature."

We have put this proposition in a broad manner; there are, of course, numerous minor points to be taken into consideration. The

preparation of a mere list would be an enormous undertaking: we learn from Dr. David Sharp and the workers on the *Zoological Record* that there are 386,000 recent species; no one has reckoned the number of extinct species. Some such work as the "Index generum et specierum animalium," now being compiled with a minimum of support and under constant difficulties by Mr. Charles Davies Sherborn, must form the basis of any such synopsis as that here proposed. The first duty of naturalists is to help Mr. Sherborn, who works at the British Museum under a Committee of the British Association. We also have to consider what is to be done when our list is completed. First of all, it must constantly be kept up to date. It seems to us that some restriction will have to be laid upon the place and manner of publication of new specific names, and we would suggest that, when the time comes, no specific name should be recognised unless it be entered by the author at some central office, together with a properly published copy of the work in which the description appears. The name would then be checked, dated, and placed at once in the Index.

It is not contended that the acceptance of our proposal would obviate the need for a code of nomenclature. But it would be a far simpler code, free from the doubt as to whether its rules were to be retrospective; and its action would be uniform and stringent. Nor is it contended that the validity of a name carries with it the validity of a species. For the stability of nomenclature, it would be advisable to include in the list as many names as possible, and to leave to specialists the duty of deciding on the distinctness and systematic position of species. But whether our aim be the completion of an Index, the compilation of a Synopsis, or the construction of a Code, it is necessary that there should be absolute and loyal co-operation between zoologists of every kind and every country, since by this means alone can the required sanction be obtained.

The Taming of the Shrews.

We have lately received, from the U.S. Department of Agriculture, "North American Fauna, No. 10," which contains a "Revision of the Shrews of the North American Genera Blarina and Notiosorex" and a "Synopsis of the American Shrews of the Genus Sorex," both written by Dr. C. Hart Merriam, Chief of the Division of Ornithology and Mammalogy; also a paper upon "The Long-tailed Shrews of the Eastern United States," by Mr. Gerrit S. Miller, junr., of Cambridge, Mass. Here, as in previous contributions that we have received, Dr. Merriam manifests his great ability, thoroughness, and fair dealing, together with his pleasant manner of making the dryer details interesting and a clear way of putting forward his views. He supposes the short-tailed shrews to have originated in the south, while the long-tailed shrews came from the north. He does not, we presume,

postulate a separate primary origin for these genera of the sub-family Soricinæ. A boreal migration of austral forms leaving no trace behind them, save possibly part of their tails, for there are no shrews in South America, seems a strange surmise. Dr. Merriam must mean that the short-tailed forms first became specialised in the region of their now southernmost settlement, and that they have subsequently retreated northward. This may be so.

In looking through these publications, the conviction is forced upon one that "they know how to do things in America," and one wonders what work will be left for the poor fellows of the next generation. So far as North America is concerned, at any rate, there will be no new species to discover nor any work to be done in unravelling synonymy, for this is all done so thoroughly by the writers of these monographs. They know, too, how to print books in America; in this, as in their other Government publications, both the paper and type are all that can be desired, and might well be commended to the notice of the "Printers to the Queen's most excellent Majesty."

The Antarctic Fauna.

The affinities of the Antarctic Fauna have recently been discussed from two points of view. On the one hand, Haacke and Wallace, impressed by the primitive character of many of the inhabitants of the southern terminations of the continents, have argued that all the forms of life originated in the northern hemisphere, and thence gradually worked their way to the south. On the other hand, the advocates of a southern continent have pointed out resemblances between the animals of Australia, the Cape, and Patagonia, and explained these as due to descent from a common Antarctic fauna. To these Dr. John Murray has now added a third speculation, which he explained in his lecture at the Royal Institution on February 28. In a recent memoir published by the Royal Society of Edinburgh (*Transactions*, vol. xxxviii., pt. 2, no. 10, 1896, pp. 343–500, 1 map), he has given a full list of the deep- and shallow-water marine faunas of the Kerguelen Region of the Antarctic Ocean. This list shows that there are in the colder waters of the northern and southern hemispheres a considerable number of identical or closely-allied species, which are separated from each other by the tropics. As Dr. Murray puts it, "the marine faunas towards either pole are genetically more closely related to each other than to any intervening fauna"; but it is also held that very few of the deep-sea species are universally or even widely distributed, and the community of species between the Arctic and Antarctic Oceans is, therefore, a very striking fact. It is this difficulty which Dr. Murray's theory is founded to explain. According to this, throughout Palæozoic and early Mesozoic times, life was restricted to the shallower parts of the oceans, which had a uniform temperature throughout, probably of about 70° Fahr.

Then, towards the end of the Mesozoic period, cooling set in at the neighbourhood of the poles, causing colder water, containing a more abundant supply of oxygen, to flow into the deeper seas, which were thereby rendered habitable. At the same time, the cooling of the polar seas so changed the conditions that the fauna of the shallow waters was impoverished and rendered sluggish, causing evolution to take place there more slowly than in the tropics. It is thus that Dr. Murray explains many of the main facts in the distribution of polar marine life. The chapter of the memoir in which these views are announced is full of original and most suggestive ideas.

The theory of a gradually cooling globe has often been applied to explain the puzzles of zoological distribution, but we still doubt its value. When we turn to the list of ninety species which are common to both the extra-tropical regions, we find so many are primitive types—such as the eight sponges or the two echinoids, or are forms which live attached to floating wood—such as two of the three lamellibranchs; while, moreover, groups of which the range of species is proverbially world-wide, such as the Ostracoda and Bryozoa, are so largely represented (by eight and sixteen species respectively), that we feel less convinced by the argument. Dr. Murray is perfectly candid as to the fact that the occurrence of Palæozoic glaciations would have been fatal to his views. And no one who saw Professor Edgworth David's series of photographic lantern slides of the Permo-Carboniferous Glacial Deposits of Australia at a recent meeting of the Geological Society can have any doubts as to the authenticity of Palæozoic glaciations. Professor David's demonstration shows that we can expect no help from a gradually cooling globe in solving the distribution of life, since at least Carboniferous time.

Two Geographical Reconstructions.

The past and present distribution of European mammals, and the light thrown thereby on the ancient geography of the Mediterranean district, are discussed by Dr. R. F. Scharff in a valuable paper in the *Mémoires de la Société Zoologique de la France* (viii., pp. 436–474; 1895). Taking, in the first place, the living and extinct indigenous mammals of Ireland, the author shows that they all inhabited Great Britain during the Forest Bed Period, while one at least—the fox—was living here in the time of the Red Crag. The mammals which are found—recent or fossil—in Great Britain, but not in Ireland, began to arrive in our area at the time of the Forest Bed, and can be traced through the Pleistocene deposits. These facts are used by Dr. Scharff, with much force, in support of the view enunciated by him in a previous paper (*see* Natural Science, vol. vi., pp. 147, 148; March, 1895), that the Irish fauna reached that island in Pliocene times. For if, he argues, the prevalent view of a Pleistocene continental connection be held, how is it that none of the animals of the second division were able to reach Ireland?

It is interesting to note that Dr. Scharff traces nearly all the mammals he discusses to an ancestral home in Central or South-Western Asia. Only a few—as the reindeer and the varying hare—are believed by him to have originated in the high north. Maps of the Mediterranean area, as it is supposed to have been at the time of the migration of the animals into Western Europe, show a continuous land-connection from Spain through Morocco, Algeria, Tunis, Sardinia and Corsica, Sicily, Southern Italy, Greece, and Asia Minor, while a Saharan sea communicates with the Mediterranean across Tripoli. The Black Sea joins the Caspian, the Caucasus and the Crimea being united by an isthmus.

Turning to the broader relations of the European fauna, Dr. Scharff advocates a Holarctic Region as defined by Dr. Hart Merriam (*see* NATURAL SCIENCE, vol. v., pp. 53–57; July, 1894). The Holarctic fauna is shown to have much greater affinity with the Oriental than with the Ethiopian; many types now characteristically African are believed to have originated in the Oriental Region.

Meanwhile Mr. C. Hedley has been working at similar problems at the Antipodes (*Proc. R. Soc. N.S.W.*, 1895, and *Ann. Mag. Nat. Hist.*, 1896, pp. 113–120). While feeling bound to admit the affinity between the animals and plants of the far southern lands, he does not consider that the facts will bear so heavy a superstructure as the antarctic continent advocated by Mr. H. O. Forbes (NATURAL SCIENCE, vol. iii., pp. 54–7; July, 1893). Mr. Hedley rejects also Professor Hutton's Pacific Bridge from Chili to New Zealand, on the strong ground that the intermediate islands show no South American affinities. His own explanation is, "that during the Mesozoic or older Tertiary, a strip of land with a mild climate extended across the South Pole from Tasmania to Tierra del Fuego, and that Tertiary New Zealand reached sufficiently near to this antarctic land without joining it, to receive by flight or drift many plants and animals." He therefore takes a middle position between the continent-builders and the stern, unbending advocates of the permanence of oceans. Mr. Hedley's caution is a virtue that needs imitation by many recent advocates of "Antarctica," who have mingled with the valid reasons for their belief many that are untenable and contradictory. They are too apt to misapprehend or to ignore the evidence of palæontology, and do not recognise the inadequacy of most of the evidence supplied by groups whose past history is unknown. In these speculations, moreover, the time-element is too often neglected. To prove a former land-connection between distant continents, the occurrence in both areas of a number of more or less related animals and plants is cited; but these common forms may include flightless birds clearly of recent origin, mammals of early Cainozoic type, fish and reptiles of undoubted Mesozoic affinities, and invertebrates that may be of still remoter antiquity. Now, granting a former land-connection—when did it exist? Was it of Jurassic, Cretaceous, Eocene, or even

later age? There are, we gladly admit, writers who recognise and allow for these difficulties, but the case for "Antarctica" has received little support of value since Dr. Blanford's presidential address to the Geological Society in 1890.

The Distribution of Plants.

The foregoing criticism applies to distributionists (chorologists, some of them like to be called) in general, and to botanists quite as much as to zoologists. In this country, at all events, few botanists seem quite to realise that plants, like nations, have a past history, which must be studied by those who would understand the existing geographical distribution of species. Scandinavia, however, is in this respect far in advance of England, for many of her botanists make a point of enquiring into the bygone distribution of each species, to compare with its present limits. Norstedt & Sons, of Stockholm, have just published a little book by Dr. J. Gunnar Andersson, entitled "Svenska Växtvärldens Historia i korthet framställd," which contains 106 pages, a map, and 53 text-figures. Herein, Dr. Andersson, who has himself done good original work on the fossil flora of Sweden, gives a clear account of the leading points in the present and former distribution of various plants, illustrating the limits of certain of the species by maps. No doubt the relation of past to present distribution is a far more involved question in Britain; but before long we ought to be able to put together a similar, or even fuller, history of our flora. On comparing Dr. Andersson's lists of fossil plants found in Sweden with those found in England, one is tempted to speculate on the curious absence of certain species from one or the other country. It is, of course, dangerous in the present state of our knowledge to lay much stress on negative evidence; still, one cannot help noticing that some plants common in the fossil state in Sweden, seem to be absent in England, and others common in England are missing in Sweden. Before the period of naval enterprise the discordance probably was far greater than at present.

Flowers and Insects.

Lately, we have all been interested in the wail that there are no "naturalists"—except deceased. On reading Professor Plateau's recent contribution to the *Bulletin de l'Académie de Belgique*, xxx., November, 1895, "Comment les Fleurs attirent les Insectes," one can almost believe that it is better not to be a naturalist—at least if one has to suffer many such processes of disillusioning as this memoir reveals. The Professor's experiments must be quite a shock to those who have been brought up in the idea that the lovely hues of flowers are due to insects, for he concludes that neither the form nor the colours of the blossoms are the means of attraction. There are naturalists and naturalists (we are sure Mr. Thiselton Dyer will forgive us for making so outrageous a statement), and some have for long

maintained the conclusion Professor Plateau deduces from the experiments to which we refer. It is quite a comfort to find that we may still believe what we actually see, viz., that flowers *do* attract insects. After all, it is only "the vexillary function of Delpino" that disappears, and so we may hope that naturalists will continue to exist in spite of the views of the Director of Kew. The experiments of the Professor at Gand are somewhat of the nature of a reprieve.

A Poisonous Orchid.

Cypripedium spectabile, a relative of our rarest native orchid, the lady's slipper, and one of the prettiest of the family, as well as the oldest in cultivation, has recently been proved remarkable for something besides its geographical distribution. It has long been known from North America, where it grows in peat bogs and tamarack swamps, from Nova Scotia to Minnesota, and in all the States east of the Mississippi. Quite recently, however, it has been found in the western provinces of China, thus adding another to the list of species to which Asa Gray drew attention as indicating so striking a relationship between the floras of the Atlantic States of North America and Eastern Asia. In part vii. of the *Minnesota Botanical Studies*, issued by the Geological and Natural History Survey of the State, a short paper by D. T. MacDougal confirms the presence of poisonous properties in the leaves and stems of adult plants of this and two other Cypripediums. Experiment showed an irritant action on the skin similar to that caused by handling various species of *Rhus*, the poison ivy. The poison was found to reside in the glandular hairs, in which the secretion, of an oily nature, is deposited in the same manner as in the Chinese primrose, that is, between the cell-wall and the cuticle of the terminal cell of the hair, and is set free by the final rupture of the cuticle. It is suggested that the poisonous property is a device primarily for the protection of the reproductive portion of the plant, as the irritant action, and the amount of secretion, increase with the development of the plant, reaching a maximum during the formation of the seed-capsules. *À propos* of *Cypripedium*, the same issue contains some remarks on the genus, with an illustrated monograph of the species found in Minnesota, in which Natural Science is quoted as the authority for certain statements which, hailing originally from the *Orchid Review*, formed the subject of a "Note and Comment" in our Journal. The *Orchid Review* is apparently unknown so far west as Minnesota.

The Temperature of Plants.

"Tree Temperatures" is the title of another of the *Studies* referred to in our last Note. During the first six months of 1894, R. W. Squires made an exhaustive series of observations on the temperature of a trunk of the Box-elder (*Acer Negundo*) and the

surrounding air. Records were made three times a day, namely, between 6 and 7 a.m., 12 and 1 p.m., and 6 and 7 p.m., from January 15 to June 3, a period including the season of continued cold, the spring season, and the early summer when the thermometer had nearly reached its maximum. A careful examination of the published tables, in the light of the varying phases of vital activity through which the tree passed during the period, should yield more important results than are presented in the few remarks which accompany them. Through the entire period the tree temperature was lower than that of the air in the morning and at noon, but higher in the evening. On four days in January the temperature of both tree and surrounding air fell below 25 deg. C. The mean temperature of the tree for January was 1·31 deg. C. higher than that of the air; in February the two were identical; in March that of the tree was nearly 1 deg. C. lower, in April ·85 deg. C. higher, and in May again 1·13 deg. C. lower than that of the air. The relatively high temperature of the tree during April is accounted for, at any rate in part, by the increased metabolic activity during the development of the reproductive organs.

Relations of another kind between plants and temperature are referred to in the January number of the *Kew Bulletin*, where some account is given of the havoc played among the plants of the Royal Gardens by the great frost of 1895. Several of the examples cited give evidence of the importance of individual constitution as a factor in the problem of plant-life. In more than one case plants under shelter were killed, while those in the open survived uninjured. Bulbs seem to have come off the worst, in many instances—*e.g.*, species of *Narcissus*—the whole planting being destroyed. Alpine plants also suffered much, evidently missing their wonted covering of snow. Some of our native plants—*e.g.*, the thyme, which suffered severely—did not withstand the cold so well as many from countries with a warmer climate than Britain. On one count the "Gardens" must be congratulated. They possess their own waterworks, and being able to keep up a supply during the whole period of frost, were thus not affected by the disastrous failure of the water companies.

Technical Education in New South Wales.

The Calendar for 1895 issued by the technical education branch of the Department of Public Instruction for New South Wales shows an admirable amount of energy expended both in direct education and in the installation of a Central Technological Museum. The museum has been removed from the temporary premises in the Outer Domain at Sydney, and is now situated in Harris Street, near Redferne railway station. It contains considerably over 30,000 specimens, and its officers impart to visitors all desired information with regard to the raw products and manufactures of New South Wales. The new building rises to a height of 75 feet, and is 183 feet long and 50 feet

wide. There are three main floors, 15 feet high, divided transversely into bays 16 feet wide. The ground floor is devoted to economic geology, metallurgy, and metal work. Among the exhibits are specimens of meteoric iron, models of meteorites, specimens illustrating phenomenal geology, together with series of rocks, minerals, and fossils, for teaching purposes.

On the first floor are the economic products of the plant kingdom. Special prominence is given to those of native origin, and an earnest appeal is made for the improvement of this section, since, as the curator rather obviously remarks, "the value of our unknown vegetable wealth is incalculable." It is pointed out that the native plant-foods are of interest, first, because every year increases the difficulty of learning about the articles utilised by the aboriginals, and, secondly, because in the more inhospitable portions of the colony it is often desirable that people should be acquainted with the food-products of the bush, such as they are. Here is also to be seen a very popular innovation in the shape of a permanent exhibition of freshly-cut Australian native flowers. Each species is placed in a separate vase of water, and is properly labelled. The plants belonging to each natural order are placed together. Under proper care, the flowers remain fresh for a long time, and the collection is largely increased by daily gifts from visitors. As many as 300 species have been on view on one day. With the help of the Toynbee Hall Natural History Club, a similar exhibit has from time to time been prepared at the Whitechapel Museum, where it is much appreciated by the East Londoners. We commend this idea to the Botanical Department of our own Natural History Museum. Of its popularity there could be no doubt, since at least it would be necessary to reduce the temperature of one of the galleries.

The second floor of this museum contains the ethnological collection, which is limited to the weapons and dress of Australian natives and those of the South Seas. The reason for considering these specimens to be of economic importance is that the material of which they are made often suggests a utilisation which has not occurred to other men, while the method and quality of workmanship are often instructive. This floor also contains the economic products of the animal kingdom. New South Wales possesses many so-called native silk-worms, the commercial value of which requires to be properly tested. The small wild bee (*Trigona*) is an insect to which the attention of residents in the bush may profitably be directed. The wool section is naturally one of the most important features of this museum. It embraces an extensive collection of representative wools from almost every country, and in all stages of manufacture.

There has hitherto been a want of organised co-operation between the museums and the educational bodies of Sydney, but we are glad to see that, so far as technical education is concerned, the advantages of mutual assistance are fully recognised; not only are

there local museums established at Goulburn, Bathurst, Newcastle, West Maitland, and Albury, and affiliated with the Central Museum, but the collections in the Central Museum and in the Technical College have been thoroughly fused, so that the lecturers of the latter are able to draw upon the resources of the well-stocked museum. The specimens of the museum, wherever practicable, are transferred to the class-rooms for longer or shorter periods, and when there are objections to the transference of the specimens, special facilities are granted to students for the examination of them in the Museum itself. The country museums differ from the central one in having a natural history side. They receive, from the Government, mining and geological maps of New South Wales, especially those which refer to their particular district.

The energy and enthusiasm of the curator, J. H. Maiden, and his assistant, R. T. Baker, are further shown in the publication of a series of hints for the collection and preservation of raw products suitable for technological museums; these are given at the end of this interesting report, and are also issued as a separate pamphlet.

THE ILLINOIS STATE LABORATORY OF NATURAL HISTORY.

THE University and State of Illinois have undertaken an investigation of the natural history of the inland waters of the State. A handsome laboratory has been erected, a competent staff engaged, and a cabin-boat with all necessary appliances stationed on Quiver Lake. Europe has furnished two important precedents, (1) the investigation of the fresh waters of Bohemia by means of a portable zoological station, carried on by Dr. Anton Fritsch for the Bohemian Scientific Land-exploration (Landesdurchforschung), and (2), the biological station at Plön in Holstein, directed by Dr. Zacharias, and maintained by imperial and municipal contributions. Illinois has made a spirited entrance into the same field of inquiry. The most peculiar local features are the great rivers (Mississippi and Illinois), which are subject to extraordinary changes of level, forming by overflow extensive lakes, which at another time of the year shrink or even disappear. The directors expect, in the first place, to contribute to the scientific knowledge of the fresh waters of the State; secondly, to encourage economic applications of natural history, with special reference to fish-culture and river-pollution; lastly, to promote the teaching of natural history in schools and colleges.

Professor S. A. Forbes, Director of the Laboratory and State Entomologist, remarks on this last head:—"Not many years ago, biological instruction in American colleges was mostly derived from books; of late, it has been largely obtained in laboratories instead; but several years' experience of the output of the Zoological College Laboratory has convinced me that the mere bookworm is hardly narrower and more mechanical than the mere laboratory grub. Both

have suffered, and almost equally, from a lack of opportunity to study nature alive."[1]

We have before us vol. iv. of the *Bulletin* issued by the State Laboratory. It is largely occupied by a first paper on the Entomology of the Illinois River and adjacent waters, by C. A. Hart. This consists of the notes, provisional classifications, and drawings of a working naturalist. There are many facts useful to the expert, but the paper must be looked upon as an instalment of a piece of work which is still tentative and exploratory. Mr. Hart seems to go about his business in a sensible way; he is quietly gaining knowledge; he is really studying live animals, and not merely cataloguing alcoholic specimens. Of course, the best part of his work is still in the future. We trust that he will gain experience without losing breadth, and that he will not fall into any of the mechanical ways of working which are deadly to scientific inquiry. Ten years of such work would furnish materials for a treatise of the greatest possible value to natural history. We look forward with high hopes to the future of this important enterprise.

America is taking the lead in State-encouragement of natural history. Her entomologists and agriculturists are collecting information and prosecuting experiments with unprecedented activity. It is instructive to note that public money is liberally provided, and that the money comes back in the form of devastation averted from valuable crops. The encouragement of agricultural, and especially of entomological, research by the United States is an object-lesson to the whole world.

The Coelomic Fluid of the Earthworm.

In a recent issue of the *Philosophical Transactions* of the Royal Society of London (vol. clxxxv., pp. 383–399), Dr. Lim Boon Keng has published the results of an interesting investigation he has made into the nature of the fluid occupying the body-cavity of the earthworm. The large cavity in the earthworm between the alimentary canal and the outer body-wall, and communicating with the exterior by the nephridial tubules and by a small dorsal pore in each segment, is filled with a milky alkaline fluid containing albuminous and saline substances in solution, and numerous amœboid corpuscles. Large numbers of bacteria, some protozoa, and little nematode worms are familiar inhabitants of the coelomic fluid. Many of the bacteria are apparently harmless. Pure cultures of one set of them had the characteristic smell of earthworms, and Dr. Lim Boon Keng suggests that these, at least, may be symbiotic bacteria. The amœboid cells were not usually found attacking the bacteria, but when anthrax bacilli were injected into the coelome, the cells at once set about destroying them. When attacking a large parasite, numbers of cells combined to form a huge plasmodium, within which the parasite was

[1] Biennial Report, Illinois State Laboratory, 1894.

destroyed slowly. The amœboid cells apparently were unable to retain hold on a nematode when that was wriggling actively, but as soon as it became quiescent the cells laid hold of it.

The dorsal pores are not simple apertures, but are crossed by strands of muscle which act as sphincters. It was found that the application of an irritant to any part of the skin at once resulted in the discharge of quantities of the coelomic fluid, containing numerous amœboid ceils. When exposed to too dry, hot, or cold a temperature, the earthworm, until its nervous system became paralysed, discharged quantities of the fluid. As Professor Baldwin Spencer has already suggested, Dr. Lim Boon Keng admits that simple lubrication may be part of the function of this discharge. But from the abundance and activity of the phagocytic cells in it, he thinks that its chief function is to prevent the intrusion of the microbes which must be abundant in the soil and humus inhabited by earthworms.

Microbes and Butter.

In his excellent handbook on the chemistry and bacteriology of the dairy ("Milk: Its Nature and Composition." London: A. & C. Black, 1895. Price 3s. 6d.) Dr. Aikman makes a just and suggestive comparison between the work Pasteur did for the brewing industry and the bacteriological work that is required in butter-making and other dairy-work. Notwithstanding the efforts of county councils and the dissemination of various excellent handbooks like this by Dr. Aikman, it seems difficult to impress on the British farmer the necessity and the commercial advantage of a knowledge of microbes. Before Pasteur worked at the yeasts of beer and wines, the same uncertainty reigned in breweries that now rules the dairy. Wort treated in apparently the same way, and made from identical qualities of material, sometimes would produce an agreeable beer, sometimes a ropy or stringy or sour decoction. Pasteur isolated the microbes that caused the different kinds of fermentation, and showed how to prepare pure cultures of the proper organisms, and brewing and wine-making suddenly developed into a certain and profitable industry. At the present time, when butter is to be made, the cream is soured or ripened. Sometimes the proper agreeable flavour appears, sometimes the cream becomes tasteless or rancid or otherwise unpleasant. The different processes that occur during the period of ripening are the work of different kinds of bacteria. Already one or two of them have been isolated, and it seems likely that pure cultures of butter-flavouring bacteria may soon come to be supplied, just as Pasteur produced the proper kinds of pure yeasts.

Diabrotica in North America.

In the *Journal of the New York Entomological Society* (November, 1895), Mr. F. M. Webster has an article about some forms of this

genus. The species of *Diabrotica* are among those successful forms of insect-life that make even the entomologist sympathise with the feeling of the outsider who concludes that it is a great mistake on the part of Nature to have so many kinds of insects. There are hundreds of species of *Diabrotica*: usually they are very pale green or yellow beetles, shining with conspicuous black marks, and some, if not all of them, are believed to be nasty to smell, and, what is more important, to taste. It appears that one of these species, formerly rare, has lately become very numerous in North America, and is doing great damage to the crops of maize. Mr. Webster suspects that there is a nursery of species of *Diabrotica* somewhere in Mexico. He has some sensible remarks on their colours and taste, it being believed that the nasty taste (hypothetical, so far as we know, for probably no one has had the courage to chew them, as Professor Plateau did in the case of the currant-moth caterpillar, with the result of finding it not nasty to him) protects the *Diabrotica* from being eaten by birds, while the colours amount to an exclamation of "we are nasty, and delighted you should know it." We hope Mr. Webster's paper may induce entomologists to make a better acquaintance with the facts about these beetles, so as to ascertain how much truth there may be in the fancies. This seems to be the chief object of his article. He is rather surprised that some of the species appear to be of a more retiring character, and do not advertise their nastiness—hypothetical or real. It is believed that, in South America, some species of the genus *Lema* have taken advantage of the bad character of the Diabroticas by looking like them, and so getting the benefit of not being eaten, although they are nice (according to hypothesis). Mr. Webster seems somewhat to regret that no North American *Lema* has yet been clever enough to adopt this hypocritical form of coloration. But perhaps there is something in the "environment" in North America adverse to such a line of life.

Warning Colours and Mimicry.

In a recent number (1895) of the *Journal* of the Asiatic Society of Bengal, Mr. Frank Finn, the Deputy Superintendent of the Museum at Calcutta, publishes the first of what he intends to be a series of contributions to a theory of warning colours and mimicry. This excellent first paper is the best kind of "contribution" to a theory, as it consists entirely of experimental observations and not at all of theoretical interpretations. It is the more welcome, as the theory of warning colours has been conspicuous, even among biological theories, for the disproportion between the superstructure and the base of fact. Not long after his arrival in Calcutta, Mr. Finn began a series of experiments as to the palatability of the common warningly-coloured butterflies of the district. For the more agreeable part in the series of dramas he enacted, he cast the common Babbler (*Crateropus canorus*),

a representative Indian insectivorous bird. To some of these, in freedom and in confinement, hungry and sated, he offered all sorts of brightly-coloured and hairy caterpillars and butterflies, sometimes alone and sometimes along with other less lurid creatures. This first contribution gives the results of a series of such experiments. Sometimes the birds certainly chose the plainly coloured, and presumably more palatable, forms offered them; sometimes they accepted and swallowed those which no bird of a really cultured disposition should have looked at. Perhaps, on the whole, the experiments showed a preponderating rejection of brightly-coloured caterpillars, which were frequently mauled but not swallowed. We look forward with interest to a continuation of Mr. Finn's work. We are specially glad that he has been able to correct observations upon birds in captivity by observations on those at large. It is a not unnatural supposition that birds in a cage should be in the position of the lady in the ditty who "wanted something to play with," and we do not think that it would upset the supposed relation of bright colour to unpalatability were captured birds to show a catholic taste.

The Embryology of Platypus.

In the ninth volume of the *Proceedings* of the Linnean Society of New South Wales, Messrs. J. P. Hill and C. J. Martin give an account of *Platypus* embryos, taken from the intra-uterine egg. It will be remembered by probably most people except Mr. Caldwell himself, that the material collected by Caldwell has afforded us very little information. The present observers obtained two eggs from the left uterus of a female, and they give a description and excellent figures of the results they got by surface examination and serial sections. The most novel feature seen in surface view was the extent to which segmentation had proceeded, although the embryo was still lying almost flat on the surface of the yolk. Seventeen somites were formed, and the only trace of pinching from the surface of the yolk was a very shallow head-fold. The hind-brain showed four well-marked neuromeres, corresponding to those described by Orr for the hind-brain of the lizard, with the difference that in the lizard the medullary folds have met when the neuromeres appear, while in *Platypus* the hind-brain is still flat at this stage. An interesting feature in the embryo was that the wolffian duct appeared to have an ectodermic origin.

I.

The Horizontal Pendulum.

SEISMOGRAPHS based on accurate and scientific principles have now been in use for about fifteen years, and we have learnt much from them regarding the movements of the ground during an earthquake shock. Far beyond the disturbed area, however, deflections of delicate instruments, such as magnetometers and levels, have occasionally been observed, and the times at which these disturbances have occurred leave little doubt as to their connection with severe but distant earthquakes. Chiefly within the last six years, instruments of still greater sensitiveness have been constructed, and by their aid it seems not impossible that we may be able to register the pulsations of violent earthquakes to whatever part of the world they may extend. One of the most valuable of these instruments is that known as the "horizontal pendulum," the fundamental principle of which has been

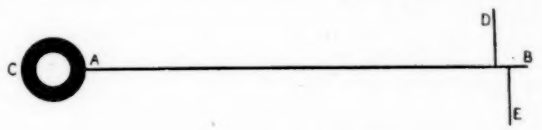

FIG. 1.—HENGELLER'S PENDULUM.

independently discovered no less than eight times within a period of sixty years.

The first occasion seems to have been in 1832, when the instrument represented in Fig. 1 was constructed by L. Hengeller, a student in Munich. The following is the description of it by his teacher, the astronomer Gruithuisen (14): "It consists of a horizontal lever, A B, of brass, on which is fixed at one end a brass ball, C, as a weight; D is a fine wire by which the lever is suspended; instead of the counterpoise, the other arm of the lever is fastened to the floor by the wire E; and the instrument becomes the more delicate the nearer the wire D comes to the wire E. The ball, C, can oscillate only horizontally, and," though the statement should perhaps be received with caution, "is visibly (according to Hengeller's experiments) attracted by a cannon ball."

Thirty years later, in 1862, a similar pendulum was devised by

Perrot (10). In 1869, again, an instrument closely resembling Hengeller's, but with improvements in detail, was made by Zöllner (16). To this astronomer is due the credit of first successfully proving and drawing public attention to the value and extraordinary delicacy of the horizontal pendulum.

It may be well, perhaps, at this stage to explain briefly the principle of the instrument. Let D and E (Fig. 2) be the two points of support, E H a vertical, and D H a horizontal line, the angle D E H being greatly exaggerated in the figure. It is obvious that the pendulum, in a state of rest, must always lie in a vertical plane passing through the axis of rotation D E; and also, in order to be in stable equilibrium, that the centre of gravity of the ball and rod must lie on the side of the vertical E H towards which the axis E D inclines.

Now, suppose the point D to receive a very slight displacement (small even compared with the length D H) in a direction perpendicular to the plane of the paper. Then the whole pendulum will turn through the same angle as the line D H does, and, obviously,

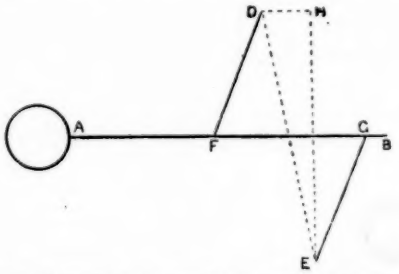

FIG. 2.—THE PRINCIPLE OF THE HORIZONTAL PENDULUM.

the smaller D H is the greater will be the angle of deflection corresponding to a given displacement of the point D. Again, the longer the vertical line H G (which is practically equal to D G) the greater will be the displacement of the point D due to a given tilt of the axis. Hence, the smaller the angle D E H, the greater will be the deflection of the pendulum caused by a given tilt of the ground; in other words, the greater will be the magnifying power of the instrument.

If the displacement of the point D is not perpendicular to the plane of the paper, it may be resolved into two components, one parallel and the other perpendicular to that plane. The former component merely alters very slightly the horizontal distance between the points of support, and therefore produces a minute, and in most cases hardly perceptible, change in the sensitiveness. The latter component is that which causes the pendulum to be deflected. Thus, the instrument is only adapted for measuring tilts of its axis in a direction perpendicular to the plane of rest. To measure a tilt

completely, it is necessary to have two such pendulums placed with their planes at right angles to one another.

Returning to the history of the horizontal pendulum, another change was made, again without any knowledge of previous work, by the Rev. M. H. Close, of Dublin (1, 7). In 1869, shortly before Zöllner, this gentleman constructed the pendulum shown in outline in Fig. 3. A B is a rod, suspended by two fine wires A C and B D, the axis, C D, being, as before, very nearly vertical. The only difference in principle between this form and Hengeller's is that the centre of gravity of the rod lies between the two points of attachment, A B, of the wires, and consequently the points of supports, C D, are both above the rod. In Hengeller's pendulum, the centre of gravity lies outside the line joining the points of attachment, and the points of attachment are therefore on opposite sides of the rod. Precisely similar in principle to Mr. Close's form is the new bifilar pendulum of Mr. Horace Darwin (3, 4, 5), which is a modification of the celebrated instruments erected in the Cavendish Laboratory at Cambridge in 1880 and 1881 (2).

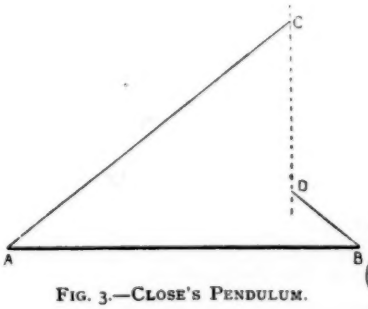

FIG. 3.—CLOSE'S PENDULUM.

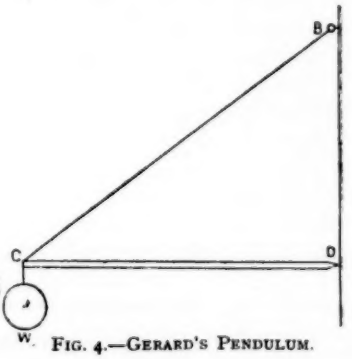

FIG. 4.—GERARD'S PENDULUM.

In the year 1851, Mr. A. Gerard, of Aberdeen[1] (6), devised another form of the horizontal pendulum, represented in Fig. 4. C D is a long beam, ending at D in a sharp point, which rests against the wall of the room in which the instrument is erected. B C is a copper wire, at one end carrying a heavy weight, W, and at the other fastened to a ring in the wall at B, so that the line B D is very nearly vertical. Forty years afterwards the same idea occurred to Professor J. Milne (8, 9), who has made much use of a similar, but smaller and more delicate, instrument, in connection with his admirable researches on earth tremors in Japan.

In 1888 and 1889 the horizontal pendulum underwent a further modification at the hands of the late Dr. E. von Rebeur-Paschwitz, of Merseburg, Saxony (4, 11, 12, 13), who removed the fine wires, or

[1] For the reference to Gerard's paper I am indebted to Kennedy's "A Few Chapters in Astronomy" (7), a valuable work which deserves to be better known.

springs, used by Zöllner, and replaced them by agate cups resting on steel points. As this instrument is one with which some most valuable investigations have been, and are still being, made, I propose to describe it in some detail. I will conclude this section with the following summarised classification of the different forms of the horizontal pendulum (quoted from the Report of the British Association Earth Tremors Committee for 1895):—

"1. The pendulum in which the rod or mirror is suspended by two wires. These may be again sub-divided: (*a*) The pendulums of Close and H. Darwin, and practically also of Delaunay, and Lord Kelvin and the Darwins, in which the centre of gravity of the rod or mirror lies between the two points of attachment of the suspending wires; (*b*) The pendulums of Hengeller, Perrot, and Zöllner, in which it lies outside them.

"2. The pendulums of Gerard and Milne, in which the rod is supported by one wire and on one steel point.

"3. The pendulum of von Rebeur-Paschwitz, which is supported on two steel points."

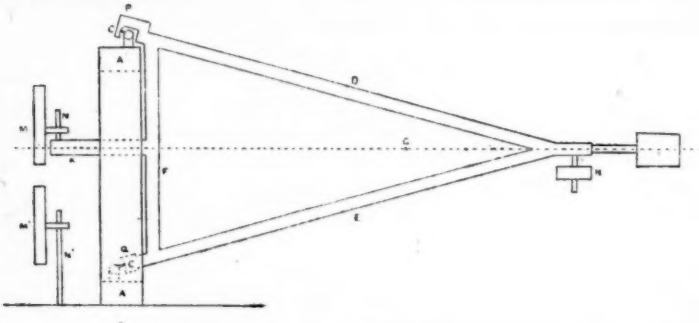

FIG. 5.—PENDULUM OF VON REBEUR-PASCHWITZ.

It should be added that Professor Ewing's horizontal pendulum seismograph, though constructed (in 1880) for a different purpose, also serves for indicating slow tilts of the ground. It resembles closely the last-named instrument.

The Horizontal Pendulum of Dr. E. von Rebeur-Paschwitz.—An outline of the pendulum and stand which supports it is given in Fig. 5. The pendulum, D E F, is made of thin brass tubes, and is in the form of an isosceles triangle. Two of the tubes, D E, are prolonged beyond the base of the triangle, and to the ends, P Q, agate cups are attached. These rest against two extremely fine steel points, C C, projecting from cylinders which rotate very stiffly about horizontal axes. The cylinders are attached to a strong rectangular frame, A, screwed to the stand, B, of the whole instrument. The upper steel point is directed away from, and the lower one towards, the centre of gravity, G, of the pendulum, and there is thus no tendency to slipping between the steel points and the agate cups. The line joining the steel points, or axis of rotation, is 68 mm. in length, and though very

nearly vertical, leans, of course, slightly in the direction towards the centre of gravity of the pendulum.

From the tube F, a rod, K, projects outwards through the frame, A, and carries a mirror, M, capable of rotating about the vertical rod, N. The mirror is shown in section in Fig. 5, and its plane is perpendicular to the axis of the pendulum. As it is situated somewhat above that axis, the centre of gravity of the pendulum is brought back to the axis by adjusting the movable weight, H.

In the direction of the axis, and about $4\frac{1}{2}$ metres from the pendulum, is placed a petroleum lamp, the light from which emerges through a vertical slit in the case enclosing the lamp. It then passes through a lens in front of the mirror, M, is reflected by the mirror, passes through the lens again, and by its means is brought to a focus on a graduated scale or photographic recording apparatus placed beside the lens. If the pendulum is deflected through any angle, the ray of light reflected by the mirror, M, is turned through twice that angle, and the distance through which the line of light is displaced along the scale is a measure of the pendulum's deflection.

If the movements of the pendulum are to be recorded continuously by means of photography (12), the scale is replaced by a cylinder with its axis horizontal and at right angles to that of the pendulum. Round this cylinder is wrapped a sheet of sensitive photographic paper on which the light is received after passing through a horizontal slit in the case enclosing the recording apparatus. The image of the source of light would naturally be a vertical line, but a cylindrical lens is placed in front of, and parallel to, the cylinder, and this reduces the line to a bright point of light. The cylinder revolves by clockwork, and the point traces its course as a narrow line on the sensitive paper.

To determine the form of this curve, a base-line is provided by means of a mirror, M^1 (Fig. 5), immediately below the other. This is attached to the stand, B, of the pendulum, but is at the same time capable of adjustment. The light proceeding from the slit of the lamp falls on both mirrors, M and M^1, and, the latter being fixed, the light reflected by it traces a straight line on the sensitive paper. The varying distance of the curved line from the base-line thus affords a measure of the pendulum's deflection. A small screen is placed immediately below the point of light from the fixed mirror, and is raised by the clock once every hour and kept up for five minutes. The base-line being in this way broken at hourly intervals, the time can be determined at which any particular disturbance of the pendulum has taken place.

The stand, B, of the pendulum rests on three foot-screws equidistant from one another. One of them is immediately beneath the axis of the pendulum, and the line joining the other two is at right angles to the axis. The screws are carefully worked and of known pitch, and are provided with large graduated heads. If one of the

screws be turned through any angle, the magnitude and direction of the tilt given to the stand can be easily determined. The first screw tilts the axis of rotation in the plane of the pendulum, and its object is, therefore, to regulate the sensitiveness of the instrument. If either of the other two screws is turned through a known amount, the angle through which the axis of rotation is tilted in a perpendicular direction is ascertainable, and the corresponding deflection of the point of light on the scale or photographic paper can be measured.

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II.

On a Proposed Classification of the Pelecypoda.

OF all the groups of Mollusca, the Pelecypoda (or as some would spell it, Pelekypoda) have ever proved the most difficult to classify, being nearly as maddening as the Bryozoa, and one system after another has been tried, only to be laid aside as inadequate. The shape of the shell, the muscular impressions, first proposed by Lamarck (1), the openings in the mantle (Cuvier, 2), and the presence or absence of siphons (D'Orbigny, 3) have all been abandoned as bases for classification. The methods most in favour at the present day are, perhaps, those based on the characters of the hinge or on the structure of the gills. The former, which, it is interesting to note, formed the principle of one of the earliest arrangements proposed, that of Martini (4), is the one relied upon by Neumayr (5, 6), and now by Dall (7, 8), and naturally is regarded with favour by palæontologists.

The gill-structure was first suggested as a possible basis by Ray Lankester (9). Fischer (10) attempted to apply it; but, since he was misled by the superficial appearances of these organs, his method has proved, if possible, less satisfactory than that based on the presence or absence of siphons. Pelseneer (11–13), however, closely followed by Menegaux (14), carried the matter further, and basing his divisions on the intimate structure of the gills, while taking into account the other features of the anatomy, produced a scheme which appeals more especially to the biological student of recent Mollusca.

Primâ facie, one would be inclined to argue that the animal, which moulds the shell to its soft body, was a more important item than the test so formed, and although, as Dr. Dall points out in his preliminary paper (7), it is not correct to speak, as many do, of the shell as a mere secretion of the mantle—its development being quite as much influenced by the physical forces surrounding it as the mammalian tooth—at the same time the modifications of the hinge, being so obviously the outcome of the mechanical requirements of the animal to protect itself in its surroundings, can hardly be so reliable a guide to the true affinities as some of the vital organs of the mollusc itself. Who, for instance, acquainted with the shell alone of *Arctica*

[=*Cyprina*] *islandica* would separate it from the Veneracea? When, however, one of the foremost of living conchologists, and a careful naturalist of world-wide reputation like Dr. Dall, comes forward with a scheme founded primarily on the hinge-characters, it is natural to turn with expectancy to his paper in anticipation that some clue to the problem, and one satisfactory to the palæontologist, has been found. Dr. Dall, however, cannot, and does not, attempt to satisfy the demand for an infallible scheme based on the hard parts; all he can do is to adopt "such an arrangement as shall," in his opinion, "best express the relation of the groups, and at the same time take into account those characters which are available for the palæontologist"; and of his three principal groups, he says, "they were based, not on the forms of hinge-teeth as such, which would have brought together many incongruous forms and separated near relatives, but on the development of a general type in each case, to which, in spite of present superficial incongruities, the pedigree of existing genera could be referred."

At the same time, there is no doubt but that a very great deal more has yet to be made out concerning the morphology of the hinge in the Pelecypoda. To begin with, the nomenclature requires overhauling; there are instances in which teeth are set down as cardinal that would appear to be lateral in origin although cardinal in position, owing to the contraction of the hinge area, as in some of the Veneracea, while secondary laterals have in some cases been developed in order to prevent the valves from shifting when closed. This is partly recognised by Dall, who calls these latter, if well developed—"teeth," and, if less pronounced—"laminæ." Of course, it is at times extremely difficult to say where the line should be drawn between a slight thickening or shelly prominence and a tooth, for these vary even in the species of a genus, and hence the necessity for examining every species of a genus, as Dr. Dall has evidently in many instances done, and not the typical forms only.

[Since the above paragraph was written, a most valuable paper by M. Félix Bernard (15), of the Paris Museum, has come to hand, showing that he has recognised the importance of these very points, and is dealing with the hinge in its morphological aspect. *See* NAT. SCI., viii., p. 153.]

The necessity for a clear understanding on the terminology becomes all the more important when use is made of formulæ such as those of Steinmann, for unless carefully and consistently drawn up, they are apt to lead, when employed for purposes of comparison, to erroneous results. *À propos* of these formulæ, it is interesting to note that the principle is much older than Steinmann's work (19), having been put into practice by Professor H. G. Seeley in 1864 (16) as an extension of the idea foreshadowed in Woodward's Manual (18).

On the whole, Dr. Dall may be said, in his classification, to approach the question rather from the standpoint of the systematist,

just as Pelseneer, with whose scheme it naturally stands in contrast, attacked the problem more from the general biologist's point of view; and had it not been that they thus advanced from opposite sides, the results obtained would probably have been more in accord, since both naturalists are equally emphatic in stating that, while their terms are borrowed from one particular feature, their classification is the result of a consideration of the totality of characters.

Dr. Dall's proposed classification was first shadowed forth in 1889 (7), in an article that deserved far more attention than it appears to have received. He has now (8) revised and elaborated it, yet retaining the general principles on which it was founded.

He begins by dividing the Pelecypoda into three main orders, which he considers proceeded more or less on parallel lines, viz.:— I. Prionodesmacea. II. Anomalodesmacea. III. Teleodesmacea. These he holds to be all descended from a prionodesmatic radical or prototype, the first and second orders retaining more evident traces of their origin than the third.

This triple ramification from a common stock certainly seems to be an advance on the more simple phylogenetic tree which satisfied the Belgian malacologist, and more in accord with the fossil evidence. At the same time, it is premature to assume that Pelseneer's tree can only rank as an "intellectual weed," to quote Dr. Sollas's happy characterisation of some such productions. Neumayr (5) started with an order, Palæoconchæ, that comprised an assemblage of ancient fossil forms of very various description, with which he apparently did not exactly know what to do, and from which he derived his other groups. Dall has no difficulty in showing that this group is nothing more than a convenient cloak for ignorance; nevertheless, although he rightly avails himself of it as a matter of temporary convenience, he gives it no place in his scheme.

The Prionodesmacea correspond to the old section Asiphonida, as given in Woodward's Manual (18), with the addition of *Solemya*, and they thus include the oysters, scallops, arks, and all pearly shells.

The Anomalodesmacea embrace the Anatinacea, Eusiphonacea (or Clavigellidæ), and Poromyacea (or Pelseneer's Septibranchia, plus Verticordiidæ and Euciroïdæ).

The remaining forms are, of course, relegated to the third order, Teleodesmacea.

When the subdivisions of these three principal groups come to be examined, much matter for criticism is revealed.

The Prionodesmacea are first divided into (I.) those without teeth in the hinge, and (II.) those with teeth, the latter being further portioned out into:—

A. Taxodonta (Nuculacea, Arcacea).

B. Schizodonta (Pteriacea [=Aviculidæ], Ostracea, Naiadacea, Trigoniacea).

C. Isodonta (Pectinacea, Anomiacea).

It is a minor point that section I. comprises the Solemyidæ alone, which are thus made equivalent in importance to the whole of the rest put together, whereas their anatomical affinities are nearest those of the Nuculidæ,whence it would, perhaps, have been better to have let them rank as a group of equivalent value to those in section II., under some such name as "Lipodonta."

Coming to the next group, Taxodonta, we find two "superfamilies" (by which term our author appears to indicate something slightly less in importance than a suborder) linked together, yet differing so considerably, not only in their anatomy, but also in their hinge-characters, that the fact of both possessing a great number of teeth in a row seems, in the light of modern biological research, hardly sufficient justification.

A more startling surprise, though, is the association of such diverse forms as are here assembled under the section "Schizodonta." Similarity of hinge-characters there certainly is none, nor, perhaps wisely, has a definition of the section been vouchsafed. *Ostrea*, when having traces of dentition, is Taxodont; in the Unionidæ alone are three types of hinge. Nor is a bond to be found in the gill-structure, the Trigoniacea being Filibranchia, while in the Ostreacea and Pteriacea the gill is more or less reticulate, and in the Naiadacea exhibits a very complicated structure. In Pteriacea and Ostreacea the ventricle is ventral to the rectum, in the others it is traversed by the latter. In short, it is difficult to see what connecting link exists between them. The sole reason that seems to have actuated Dr. Dall would appear to be that *Philobrya*, which is "probably allied to *Avicula* . . . passes through a *Glochidium* stage, thus adding another link to those which connect the Aviculidæ with the Naiades," and that where the Unionidæ go Trigoniæ must follow. Among the Naiadacea it is to be noted that Dall tentatively places the Megalodontidæ, induced, seemingly, by their resemblance to some forms of North American *Unio*. The statement concerning *Philobrya*, which doubtless will be confirmed by future observation, is extremely interesting but not conclusive. Parasitism in the young stage does not necessarily imply affinity, any more than it does in the adult.

Concerning the Isodonta, despite those who lay stress on the prodissoconch, it may be observed that Pectinacea and Anomacea do not, on anatomical grounds, seem happily yoked together. It is worthy of remark that in the young state the byssus of Anomia issues from the *ventral* margin of the shell, as in *Arca*.

Dr. Dall points out, and we believe the observation is new, that in its juvenile stages *Spondylus* has a Taxodont hinge; but he does not, as in the similar case of Chamacea, propose its removal from its allies on that account.

The Dysodonta, containing the well-defined Mytilacea, call for no remark beyond the expression of belief that the restitution to them of

the Dreissensiidæ, which Pelseneer banished to the company of the Unionidæ, is justified by their embryonic history.

The families placed under the order Anomalodesmacea would, anyhow, be near allies, and hence it is no great concern that Dall classes the Septibranchia with the Anatinidæ. It is an old quarrel; nevertheless the anatomists would seem to have the best of the argument, for the existence of the septum in the one group, be its morphological origin what it may, is surely sufficient justification for the separation of the two.

The third order, Teleodesmacea, practically corresponds to the Eulamellibranchiata of Pelseneer: its subdivisions, five in number, are, however, very different. Of these the Pantodonta (Allodesmidæ), the Cyclodonta (Cardiacea, Tridacnacea, and Isocardiacea), and the Asthenodonta (Myacea and Adesmacea [=Pholadidæ, etc.]) call for no comment. Nor do the Telodonta, beyond the remark that the Solenacea, here included with Veneracea, Tellinacea, and Mactracea, have hitherto usually been classed with the Myacea. The remaining group, Diogenodonta, comprises Pelseneer's Submytilacea (minus, of course, the Unionidæ and their allies, Ætheriidæ, removed to the Schizodonta, and Dreissensiidæ, with Modiolopsidæ referred to Dysodonta) and the Chamacea with their allies the Rudistæ.

The latter surprising innovation has been introduced by Dr. Dall on the ground that "*Echinochama* has a free nepionic stage, in which it has the form, hinge, and other characters of *Cardita*." Boehm (17), agreeing, he says, with Steinmann, considers, on the other hand, that the prodissoconch in question most nearly approaches the group of *Astarte studeri*. Granting that this nepionic stage proves the affinity of the Chamacea to the Carditacea (or Astartacea), it is still doubtful if this sanctions the wide separation of these two, and the Rudistæ, from their allied forms the Cyclodonta; might it not equally be held of sufficient force to warrant the inclusion of the Carditacea in the Cyclodonta? True, it would have spoilt the present definition of that group; but the assemblage itself would have been rendered no more anomalous than is Schizodonta. Moreover, it is worthy of note that the prodissoconch in *Chama* itself by no means resembles that of *Echinochama*. Suppose it should turn out that, in spite of the strong resemblance of the hinge-teeth in the adult forms, the nepionic stages should reveal a different line of descent for each of these two genera?

Destructive criticism is of course easy; but what alternative proposal can be made when doctors differ thus? Practically none, till more is known of the anatomy and life-history of the majority instead of the minority of the forms, and till our museums contain series of specimens of all ages with properly dissected examples of the animals, in lieu of the simple shell of two or three adult individuals of each species (so-called).

Dr. Dall himself freely admits his 'provisional classification,' as he wisely is careful to call it, to be far from perfect, but maintains "that

the groups, whether well or ill, have been comparably defined," a point on which some may differ from him, and even quote against him certain passages out of his own work. Be that as it may, no one can fail to feel anything but gratitude towards him for his plucky attempt to deal with a most difficult subject—an attempt the comparative success or failure of which can only be rightly judged in the light of further researches. The subject, however, will only be properly dealt with when the systematist, the palæontologist, and the anatomist will consent to work hand in hand for the common object. It will then probably be found the safest course to name each of the larger groups after some one of its more conspicuous members, instead of fixing on conchological or anatomical features which, by their very employment, are apt, albeit quite unconsciously, to bias the opinion of whoever employs them.

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B. B. WOODWARD.

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III.

Sharks as Ancestral Fishes.

PRIMÆVAL sharks are certainly to be looked upon with great interest by the student of fishes; for recent discoveries have seemed to warrant the belief that by a better knowledge of these ancient forms the clearest light is to be thrown on the problem of ancestral fishes. The study of living sharks has been in many ways an unsatisfactory one; for, while many of their adult structures are usually conceded to be generalised and therefore primitive, other and important structural characters must be regarded as much modified. Indeed, on this account, Kupffer, Beard, Pollard, Dollo, Retzius, and Klaatsch, among others, appear to have come to regard the sharks as perhaps a more divergent group than some of the archaic ganoids. Thus, sharks appear to have lost entirely their ancient mode of reproduction—the eggs are no longer fertilised externally, "claspers" being present in the male of every recent shark[1]; the eggs are now few in number, enormous in size, and elaborate in their range of protective capsules; their developmental characters, especially those of the earliest and of larval stages, are remarkable in their adaptations.

Whether the recent sharks, in spite of their many specialised characters, are yet to be regarded as representing most nearly the ancestral stem-form of the fishes, becomes, accordingly, a doubtful, if an interesting, question. Considered on broader grounds, they have certainly a strong claim to be looked upon as primitive; the characters of their integument and skeleton, of their circulatory, renal, and nervous systems, and end organs, together with their more numerous gills and body-segments, and the simpler condition of tail and fins, stand as weighty evidence in favour of their generalised position.

On the other hand, it becomes equally clear that this problem can be settled definitely only when evidence shall be forthcoming to demonstrate that the ancestors of the living sharks were, not only clearly shark-like (*i.e.*, not approaching in structural characters the stems of other groups), but were also clearly lacking in whatever might be interpreted as the specialised features of their descendants. The solution of the entire problem, accordingly, must naturally fall to the palæontologist, and we must await the discovery

[1] Professor Lütken, in a recent conversation with the writer, stated that he had determined functional claspers in *Læmargus*.

of Palæozoic sharks in a condition that shall render the study of their structure possible. At the present time, enough at least of the fossil forms have come to light to convince so critical a palæontologist as Mr. A. S. Woodward that "if the earliest true fish could be found, it would almost certainly fall within the sub-class Elasmobranchii."[1]

Three early sharks have, within the past few years, been somewhat definitely described. The earliest of these, *Cladoselache* (*Cladodus*), is, however, as late as the Lower Carboniferous. Next to this in antiquity is *Chondrenchelys*, from the Carboniferous; and the latest, best known in the Permian, is *Pleuracanthus* (and *Xenacanthus*). Previous to the description of these early types, the Acanthodians, members of a highly specialised group of the Elasmobranchii, had alone been structurally known.[2]

The features of these early forms may now be summarised with a view of determining what evidence they afford as to the ancient descent of the sharks.

Pleuracanthus, although the latest of these types, appears, nevertheless, as the structural studies of Cope, Fritsch, Brongniart, and Jaekel have shown, to have been decidedly unlike recent sharks in many of its features. It may, perhaps, represent the advancing stem of the Elasmobranch group, which may early have given rise to Teleostomi or to the Lung-fishes. At all events, *Pleuracanthus* gives us but little clue to the structures of the primitive sharks. We find, for example, that it had nearly lost its body-armouring of shagreen[3]: that it had evolved archipterygial fins, and a diphycercal tail; that it had produced a spine, elaborately specialised, and remarkable in its anterior position. Add to these characters the fact that no more than five gill arches were present, and that the upper element of the mandibular arch had become differentiated to a degree which rendered the skull amphistylic.[4] Moreover, the most important primitive

[1] NATURAL SCIENCE, vol. VI., p. 38. Jan., 1895.

[2] Add to these *Symmorium*, a Permian form recently described by Professor Cope.

[3] That it had a body-armour of shagreen seems evident from a study of Dr. Brongniart's material in Paris, for a patch of typical denticles occurs in the male, near the base of the claspers. *Cf.* also Jaekel, *SB. Gesell. nat. Freunde Berlin*, no. 4, p. 77; 1895.

[4] To these highly-evolved features of *Pleuracanthus* might perhaps be added (1) the unsegmented character of the main body of the gill-bars (as in Lung-fishes), (2) the presence of teeth in the pharynx, (3) the greatly calcified condition of the skeleton—a feature of more or less value in connection with other advancing or specialised conditions. Two other primitive features are noted by Jaekel (*op. cit.*, p. 72), (1) the paired character of the hypobranchial, and (2) the segmented condition of the shoulder girdle. The present writer, while inclined to accept the first of these conditions as primitive, must note, nevertheless, that the copulæ of the other arches are so widely differentiated that it is clearly possible that the paired hypobranchials might as well represent adaptive structures. With regard to the second point, having, through the kindness of Dr. Jaekel, examined the Berlin material, the present writer cannot but believe that the segmented nature of the shoulder girdle is due entirely to accidental fracture.

features which could be attributed to *Pleuracanthus* are certainly present among recent sharks—*e.g.*, the possession of a spiracle, and of a notochordal skeleton.

A study of the second Palæozoic shark, *Chondrenchelys*, Traquair, so far as our present knowledge goes, affords just as little satisfactory evidence as to the primitive characters of primæval sharks. This form may indeed have been rather of a pleuracanth type, with its elongate body and tapering tail; nevertheless, it was certainly well differentiated in its skeletal characters, possessing well-marked vertebral centra and arches.

It is accordingly to the third and oldest of these ancient sharks that we have finally to turn for more definite suggestions as to the primitive characters of Elasmobranchii. *Cladoselache* has already been mentioned several times in the pages of NATURAL SCIENCE in connection especially with the primitive characters of its fins. Its remaining structures, however, are found to prove equally important from the morphological standpoint; and in the present connection a summary of its general characters may be given.

Cladoselache is at the present time to be looked upon as representing a group of Palæozoic sharks included in a single genus (perhaps two genera), and so far as the present writer is aware, in about a dozen species. These are relatively small in size, varying from two to six feet in length. They were first brought into notice about 1888 by the late Professor J. S. Newberry, of Columbia College, New York, and they have subsequently been described by Professor E. W. Claypole, Mr. Arthur Smith Woodward, Dr. Otto Jaekel, and the present writer. The material from which these fossils were originally known has now been added to the collection of Columbia College; but an abundant material, including several dozen complete specimens, some of which are admirably preserved, is now in the private collections of Dr. Wm. Clark, of Berea, Ohio, and Rev. Wm. Kepler, of New London, to both of whom belongs the credit of its discovery.

The Cladoselachidæ have been found only in the Waverly Sandstone (at the base of the Lower Carboniferous) of Ohio; they here occur in heavy, oblong concretions, which are usually obtained when weathered out of the precipitous stream-margins. A concretion contains usually the entire fish, shown in nearly every case in ventral or dorsal aspect. The only part of the animal which was subject to widely varying conditions during the period of fossilisation was the body-width; the digestive tract and the visceral cavity seem here to have been considerably inflated with gases of decomposition, and justly account for the present position of the fossil. The antero-posterior dimensions of the fish are found to be constant in the same species.

A careful series of measurements of well-preserved fossils has enabled the present writer to complete a restoration which he believes to represent *Cladoselache* in three aspects with fair accuracy,

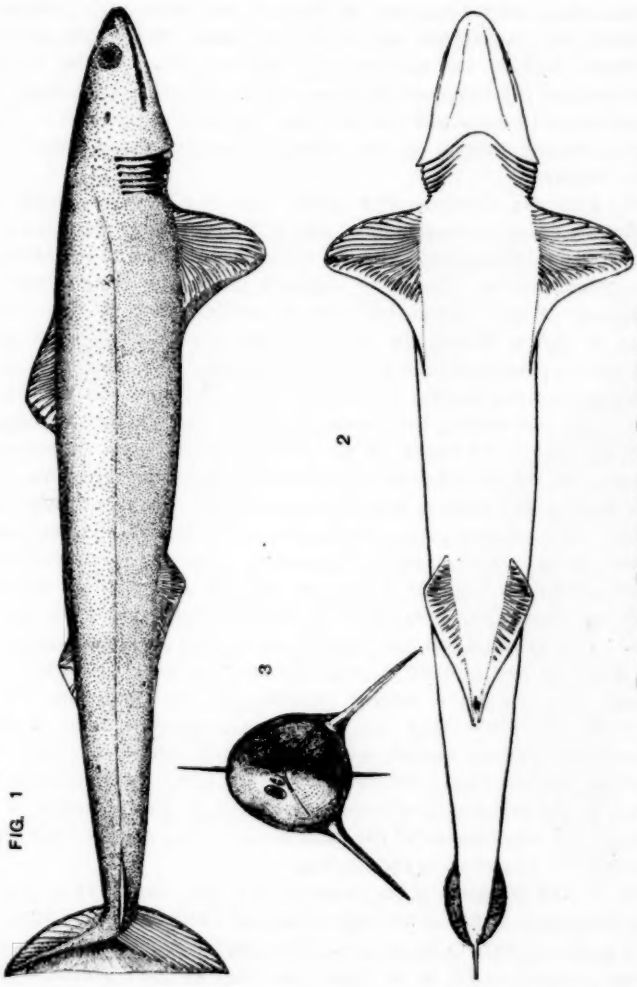
Figs. 1, 2, 3. In Fig. 2 the body-width has purposely been made narrower than in the fossils, and in Fig. 3 an attempt to present an idea of the head-view is believed by the writer to be trustworthy as to the down-turned position of the pectoral fins, and in general as to the vertical proportions. Outwardly, the subfusiform body would appear to resemble closely that of a modern shark; the fins, too, in their size and position, have somewhat of a modern look, and at the base of the tail occurs the small horizontal dermal keel of many living forms. Its paired fins served unquestionably as balancing organs, and could have had but little movement save at right angles to the plane of the fish's motion. The tail, homocercal in outline, and with its sharply-marked lateral keels, is too nearly of the outward character of the mackerel's to permit any other belief than that the swimming of the fish was active and rapid, and its shape certainly destroys the hypothesis of Jaekel, that *Cladoselache* was a form specialised to bottom-environment.

That *Cladoselache* was an exceedingly generalised shark, and that it, in fact, gives at the present time our clearest idea of the characters of the ancestral Elasmobranch, seems to be warranted by the study of the following structures:—

(*a.*) *Notochord, Vertebral Arches.*—From the mid-body region backward, a broad notochordal space may be traced in favourable fossils (*e.g.*, one discovered by Rev. Dr. Kepler, and given by him to the college at Delaware, O.); in the tail this is found to be underlaid by a well-defined tract interpretable as sub-notochordal rod. The neural and hæmal arches are also of interest; they prove to be of moderate size and thickness, extending no further outward than about one-half the vertical distance between notochord and integument. They are metameric in arrangement, *i.e.*, corresponding in number with the calcified remains of the muscle-plates, and closely resemble each other—each a tapering rod of cartilage forked at its base. Interneurals are absent, thus distinguishing *Cladoselache* from the Pleuracanthidæ and all other sharks.

(*b.*) *Branchial Arches, Jaws.*—Five gill-arches are clearly present (*cf.* Professor Claypole, *American Geologist*, vol. xv., Jan., 1895), and in a favourable specimen in Columbia College there appear to be traces of a sixth and seventh; they are interesting in their similar size, their stoutness, and in the transverse plane in which they lie. Their elements have not been clearly distinguished. The upper and lower jaws are similar in shape and size; their hinge is supported by an elongate hyo-mandibular, somewhat as in *Chlamydoselache* or *Scyllium*. In the wide space between this element and the palato-quadrate a spiracle may well have been present. The mouth was terminal; the gills possessed frilling integument, supported by fibrous rays, and the foremost of these dermal gill-frills appears to have been sufficiently large to have served as an operculum.

(*c.*) *Integument, Teeth.*—The integument, everywhere lacking in



FIGS. 1–3.—RESTORATION OF CLADOSELACHE.
A shark from the Lower Carboniferous of Ohio in side view (1), and from the inferior (2), and anterior (3) aspects.

membrane bones, is studded with minute lozenge-shaped denticles. No enamel appears to be present on the denticles of the body-region, and in a recent paper of Professor Claypole (*Proc. Amer. Micr. Soc.*, pp. 192–195; 1894) it is shown to be also absent on the teeth. The Cladodont teeth are arranged on each mandibular ramus in a dozen or more banks, each composed of about seven teeth. Of these the innermost are the largest, and of all the banks those most nearly symphysial include the stoutest and longest teeth. At the hinder mouth-margin, the shagreen denticles, here enlarged, approximate in size and shape to the smallest of the teeth (as in *Chlamydoselache*): they are also notably present in the enlarged shagreen-plates surrounding the orbits.

(*d.*) *Fins and Girdles.*—The paired fins of *Cladoselache* must be looked upon as lappet-shaped remnants of the continuous dermal fold, which is generally believed to be the ancestral condition in the history of the paired limbs. And it is certainly most interesting evidence in support of the lateral fold theory to find that in this most ancient of typical sharks the fin-conditions are precisely what one might have expected on purely *à priori* grounds. The pectorals and ventrals are entirely lacking in lobate bases. The fins lie in the plane of the fish's movement, the hinder limits of the ventrals drawing closely together in the region of the anus; their external supporting elements, the radials, extend metamerally from the body-wall to the fin-margin, and there is practically no dermal margin surrounding the fin.[1] The pectorals are clearly the more specialised: they are enlarged in size, their anterior supporting elements, thickened and blunted, forming a compact cut-water, and the elements of the fin include the compacted structures[2] of between thirty and forty metameres. The ventrals, on the other hand, retain, as Wiedersheim's work leads us naturally to expect, the more primitive characters; although they present a smaller number of metameral supports—twenty-two to thirty—they are but the elongated remains of the dermal fin-fold, whose margin is slightly blunted anteriorly, where the supporting elements have become compressed into a serviceable cut-water. In the mid-region of these fins, as also in the pectorals, it is doubtless the compression of the radials which has caused their tips to intercalate, as shown in the figures.

As to the supports of the paired fins, in the ventrals (Fig. 5) the basal cartilages, B, are as yet segmental and unfused, and no form of pelvic girdle appears to be present. The supporting elements of the pectorals are not as yet to be made out clearly; their probable dis-

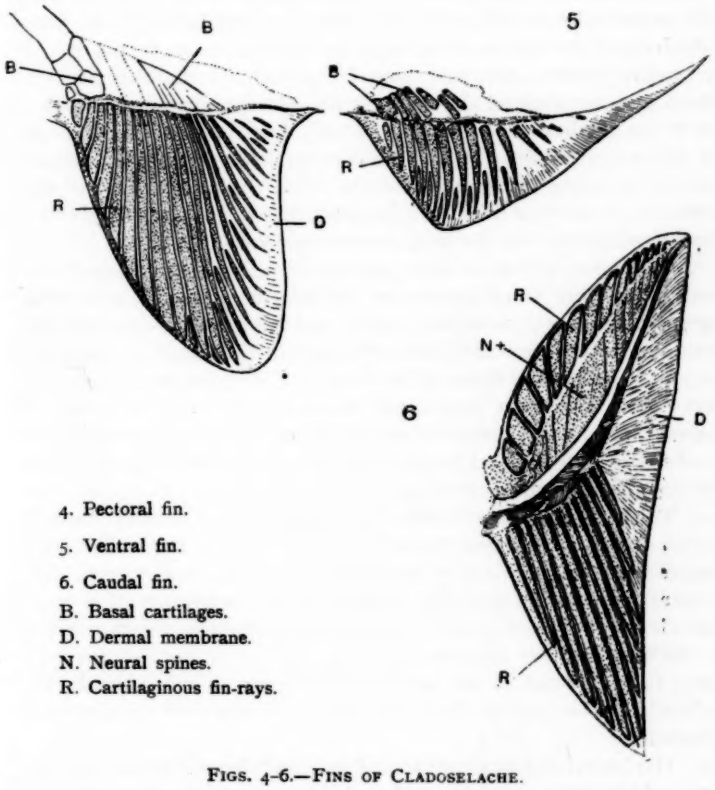
[1] This condition is nearly parallelled in the recent bottom-living Elasmobranchs; in these, however, the radials are invariably *jointed*, and their fins, great in size, are not directed downward, as the fossils indicate, to the plane of the pectorals of *Cladoselache*.

[2] The present writer has preparations which show that the intercalated radials traverse the entire fin; it is accordingly evident to him that these rays may justly be regarded in all cases as homologous structures

position is suggested in the adjoining figure (Fig. 4, B). The shoulder girdle appears to be represented in a pair of flattened "coracoid" elements.

The unpaired fins seem, as shown in a specimen in the Museum at Delaware, O., to have been outlined as in Fig. 1. Two dorsals are present, spineless, their shape corresponding somewhat to the ventrals, the foremost one slightly the larger. The latter includes the

FIG. 4



4. Pectoral fin.
5. Ventral fin.
6. Caudal fin.
B. Basal cartilages.
D. Dermal membrane.
N. Neural spines.
R. Cartilaginous fin-rays.

FIGS. 4–6.—FINS OF CLADOSELACHE.

supporting elements of about fifteen metameres, arranged similarly to the radials of the paired fins; their basal supports have not been determined.

The caudal fin is unique in its structure (Fig. 6). It is certainly of the elasmobranchian type, heterocercal, but its widely upturned tip has brought it to the limits of homocercy. Yet, although it has become specialised for its swift-swimming function, it seems evident that this fin is nevertheless of an exceedingly primitive type, more

primitive than the caudal of the most generalised of modern sharks, inasmuch as its cartilaginous supports extend from the body-terminal as far as the extreme margin of the fin, and that the hypural region of the dorsal lobe is entirely without cartilaginous or other supports (save, perhaps, actinotrichia), precisely as in embryonic protocercy.

(*e.*) *Reproductive System.*—*Cladoselache* appears to have been lacking in claspers—a condition unknown among recent Elasmobranchia,[1] and exceedingly significant, therefore, in this ancient shark. The general character of the hinder margin of the ventral fin seems at once to preclude the presence of intromittent structures; in over a dozen examples of well-preserved pelvic fins nothing has been found which could possibly be interpreted as relating to these organs. It accordingly follows, with more than fair probability, that this primæval shark had not evolved the highly-modified reproductive methods of its living kindred, but, like ganoid and lung-fish, fertilised its eggs at the moment of extrusion. In this event, it would seem exceedingly probable, judging by many analogies, that the number of the eggs deposited must have been considerable; they might accordingly have been small in size and not over rich in their yolk-supply.

(*f.*) *Organs of Sense.*—The position of the olfactory capsule has been outlined in many specimens; the position of the external narial openings appears to have been dorsal, and the present writer believes, from the study of a well-preserved mouth-margin, that its conditions in general resembled those of Notidanidæ. Well-marked labial cartilages must have been absent, and there is certainly no evidence in support of the cirrhostomial theory (Pollard) to be derived from *Cladoselache*. No lateral flap-covered canal appears to have united the cavities of mouth and nose.

The eye capsule was scarcely larger, proportionately, than in recent sharks. Its dermal investment by concentric rings of enlarged shagreen plates has already been noted by Jaekel and others. The present writer notes that this method of the protection of the eye capsule may well be regarded as a primitive specialisation, inasmuch as the integument is functioning in the defence of the delicate optic sense-bulb, instead of the underlying tissue; in other words, the sclerotic of this ancient form may not have assumed its protective characters.

The lateral line has been traced as a well-defined space between rows of shagreen denticles, and it follows, therefore, that an open sensory groove persisted, as in *Chlamydoselache*. In the fossil form, however, judging from the characteristic tail structure, this condition could not have been due to bathybial environment.

In view of the structural characters which have just been summarised, it seems to follow that in *Cladoselache* has been discovered an Elasmobranch of an exceedingly primitive nature.

[1] This condition does not occur in *Læmargus*, as Professor Lütken at first believed.

Even its specialisations are evidently of a lowly order: the differentiation of the fins, their radial supports, with their range of size and shapes clustering in the anterior fin-margin, are of this class; also the shapes of the cladodont teeth, and the large shagreen denticles surrounding the orbits or strengthening the cut-water margins of the fins; as well as the curious differentiation of the caudal fin, in its extreme degree of heterocercy and in its dermal lateral keels. The sum of the generalised features of this most ancient shark cannot, moreover, fail to place it, judging from the standpoint of the morphologist, near the ancestral stem of the Elasmobranchii. Its primitive fin-characters and lack of clasping organs, moreover, break one of the strongest barriers that have separated the primitive phylum of the Elasmobranchii from that of the Teleostomi and lung-fish. Furthermore, it is with the characters of *Cladoselache* that the puzzling structures of the Acanthodians are now most reasonably to be compared. The fin-structures of the more generalised form of *Cladoselache* are found to be clearly approaching those of the Acanthodians. The fin-spines of the latter, although clearly encased *outwardly* with purely dermal structures,[1] are, nevertheless, to be regarded as of compound origin, representing morphologically the concrescence of the radial supports in the anterior margin of the fin; and it is significant that the fin of a Cladoselachid, which has been discovered by, and is now in the collection of, Dr. Clark,[2] is decidedly of a spine-like character, nearly thrice as long as wide, its supporting elements forming a compact and Acanthodian-like fin-support.

Up to the present time the discoveries among the primæval sharks can only be regarded as the beginnings of our knowledge of these remarkable forms. As beginnings, however, they must certainly be acknowledged as already of extreme interest, demonstrating the existence of elasmobranchs of primitive and generalised characters, and enabling us to form an idea of what may have been the direct ancestors ("*Proselachia*") of the recent sharks and rays, if not, indeed, of the remaining groups of fishes.

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[1] *Cf.* Smith Woodward, NATURAL SCIENCE, vi., pp. 38–44.

[2] By whose courtesy the writer is enabled to describe and figure it in a forthcoming number of the *Anatomischer Anzeiger*.

IV.

Rules of Nomenclature in Zoology.

THE code of the German Zoological Society for the moment holds the field. It is to govern the naming of all the known animals of the world in the forthcoming volumes of "Das Tierreich." There is still, however, a brief interval during which the code is liable to be modified. It may, therefore, be made better or it may be made worse. At a recent meeting of the Zoological Society of London only one point was made transparently clear, namely, that no body of regulations on the subject of scientific nomenclature can possibly give universal satisfaction. Some speak of the law of priority as "a fetish" and "a demon," while others hold that it governs almost *jure divino* in the vocabulary of science. Some persons are taunted with being extremists who look upon themselves as the pure models of logical consistency. Some think that we ought to bury the past and bind the future, while others believe that the more justice we render to our predecessors, the more surely will our own decisions be accepted by those who come after us. Some single out for censure the very points in the German code which others think exceptionally deserving of praise.

The first rule relieves the zoologist from the obligation of paying regard to the names of botanical genera. This is in accord with Sir William Jardine's Report, approved of and adopted by Section D of the British Association in 1865, on the motion of Mr. Gwyn Jeffreys, *seconded by Dr. Sclater*. Since that time the relief in question has generally been taken for granted, as a matter of such obvious convenience, as a blessing so unqualified, that it could need no further argument. With much surprise, therefore, did one hear or seem to hear Dr. Sclater himself maintaining the contrary opinion. He urged the chances of confusion between a genus of plants and a genus of animals, especially when we are dealing with the borderland between animal and vegetable life, and he pointed out that the recent "Index Kewensis" now made it easy for the zoologist to discover and avoid the names which have been used in botany. But the "Index Kewensis" is of no avail to the zoologist working at a distance from great libraries. He is certain not to possess it, and, were it ever so near to his hand, it would tell him nothing in regard to the continuous coinage of new botanical names. In any case, in order to avoid the infinitesimal

risk of confounding a tadpole with a tulip, or even the crime of giving to a new protozoön a name already consecrated to a protophyte, it surely cannot be worth while to fetter all zoologists again with the botanical shackles from which they have shaken themselves free.

As for that part of Rule 5 which ordains that such a name as *Scomber scomber* Linn., should prevail over the later *Scomber vulgaris* Fleming, let those dispute who care. It seems to be a contest between Linnæus and Strickland, between a demigod and a hero. The repetition of sounds in *Scomber scomber* is said to be something quite excruciating to the ear. Yet in the solemn anthem, musicians have been known to favour such repetitions, the orator uses them, in poetry they occur without offence, and even our English aristocracy sometimes bears them as an added grace.

The seventh of the German rules adopts the tenth edition of the "Systema Naturæ" (1758) as the starting-point of zoological nomenclature. It is late in the day to oppose this, since here again the German Society is not leading opinion, but wisely following it. Sir William Jardine's Committee no doubt objected to the change from the twelfth to the tenth edition. They argued that the twelfth was the last and most complete edition of Linnæus's works, containing many species not found in the tenth, and that much confusion would result from the changes of names required upon an alteration of the date which had been accepted for twenty-three years. If men would measure time by some more adequate standard than the length of their own lives, they would not attribute an overweening importance to the lapse of something less than a quarter of a century. Apart from this, the objection is still upheld by some important authorities that it would be disrespectful to Linnæus to adopt the tenth edition of his work for our starting-point, inasmuch as in the later edition he himself introduced alterations in the names employed. Now, no rule is more properly and more generally accepted than this, that over names once published the author has no more authority than anyone else. Therefore, on the plea of reverence for Linnæus, the adoption of the twelfth in preference to the tenth edition of the "Systema" is simply to hold up Linnæus as an *exemplum ad vitandum*.

Those who look with horror upon the date of 1758 must be prepared for a further shock. Taken in an uncompromising manner, it is still not early enough. Professor T. Thorell ("On European Spiders," p. 8; 1870) says, "As regards *Spiders* in particular, Clerck has already 1757, in his famous work 'Svenska Spindlar, Aranei Suecici,' applied Linné's nomenclature in perfect consistency, and accordingly the denominations given by him in that work have right of priority in preference to the Linnæan." The Jardine Committee, with reference to the date of 1766, make similar exceptions as to the works of Artedi and Scopoli. Assuredly it would be easy to ascertain whether any other writings with similar claims to those of Clerck were published between the date of 1758 and that of 1751, "when

Linné's 'Philosophia Botanica' appeared, in which his new system of nomenclature was first fully and distinctly propounded." Though Linnæus on the whole towered above his contemporaries, it is a mistake to suppose that he was on a uniform level at all points of knowledge. In several special departments of zoology he was conspicuously below the specialists in those branches, and therefore in his favour to cancel the properly constructed systematic names which they were the first to give, can hardly square with the humblest, let alone any lofty, ideal of justice.

In the German rules there are some inconsistencies. A footnote to the second explains that the sentences in small print contain explanations, exemplifications, and recommendations. But the first sentence in small print attached to the third rule contains the ominous word "must," which may be explanatory, but does not read like the language of advice. The rule in question is that "Scientific names are to be regarded as Latin words." Then follows the exhortation in small print that "In words derived from the Greek the following transliterations must uniformly be employed." It is not twenty-three but fifty years ago that George Grote, the famous historian, taught us to welcome such words as Aristeides, Hêlios, Asklêpius, Korônis, and, in regard to this innovation, said, "I have approximated as nearly as I dared to the Greek letters in preference to the Latin." But the German ordinance, while not attempting to introduce the simple and much-needed service of distinguishing the long vowels from the short, would now compel us, as of old, to change EI into I, K into C, and so forth. If this is meant to be retrospective, the interference with established names will not be trifling. Such words as *Cheirocratus*, *Leptocheirus*, *Stylocheiron*, *Kallospongia*, *Kentrodorus*, will be tortured out of their accustomed form. In the interests of pronunciation an Englishman might well wish that, at least for scientific names, that silly C, the opprobrium of our alphabet, had been altogether discarded. As Grote says, "The ordinary practice of substituting, in a Greek name, the English C in place of the Greek K is indeed so obviously incorrect, that it admits of no rational justification. Our own K precisely and in every point coincides with the Greek K: we have thus the means of reproducing the Greek name to the eye as well as to the ear, yet we gratuitously take the wrong letter in preference to the right. And the precedent of the Latins is here against us rather than in our favour, for their C is really coincident with the Greek K, whereas our C entirely departs from it, and becomes an S before E, I, Æ, Œ, and Y." Further on he says, "We mar the unrivalled euphony of the Greek language by that multiplied sibilation which constitutes the least inviting feature in our own."

But, again, why should we be forced or induced in the name of Latinity (!) to use such forms as *Linnæusi* and *Möbiusispongia*, while nothing is done to prevent the naming of a genus *Platysiâgum*, a form which is neither Greek nor Latin nor Latinised Greek? Why should

we be asked to flout an excellent scholar like Henrik Kröyer by changing his *Lafystius* into *Laphystius* and worsening his *Protomedeia* into *Protomedia*? Why should we play the pedagogue to men who, without happening to be scholars, may have been first-rate naturalists? Is it not an impertinence, and a useless one, to correct their spelling which, as it stands, is characteristic and has a kind of biographical interest? Why, too, are we told that the termination -idæ in the names of zoological families is derived from the Greek -ειδης, whereas patronymics in Homer end in -ῑδης, while -ειδης, signifying likeness, is more suggestive of a picture-gallery than a family?

The fourth rule says that "Names of the same origin and only differing from each other in the way they are written, are to be considered identical." This opens the way once more to mischievous interference with established names, but the examples offered combine to give it so perplexing a vagueness, that they may haply and happily lead to its being cancelled. Thus, *moluccensis* and *moluccanus* are allowed to stand side by side, but *Fischeria* is said to be equivalent to *Fisheria*, and yet Fischer is German, while Fisher is English, and perfectly distinct genera have been designated after each of these distinct surnames.

The fifth rule permits "orthographical correction when the word is without doubt wrongly written or incorrectly transcribed." From among numerous examples offered, *Oplophorus* may be cited, which is to be corrected into *Hoplophorus*. Can anything be more superfluous? The Latins themselves were uncertain whether H was a letter or only a breathing. They fluctuated between the spelling of Adria and Hadria, of Hannibal and Annibal. Why should we, then, be more Roman than the Romans? To the breathing itself it is sometimes alleged that some of us pay but scant attention, even in classic London, and the French, who avowedly waste little time over aspirates, with much consistency give such a word as *Oplophorus* the same initial which it has in Greek. There is nothing wrong in that, and, if it were ever so wrong, what advantage is derived from interfering? Someone will next be telling the distinguished editor of "Das Tierreich" that he does not know how to spell, and that the gigantic work he is editing ought to be called "Das Thierreich."

Rule 13 declares that a specific name is to depend grammatically upon the generic name. This implies that adjectival names of species must agree with the supposed gender of the generic name. It would be far simpler to regard all generic names of animals as masculine, and thus rescue naturalists from the effects of a superstition that in Greek and Latin all words ending in "a" are feminine.

Rule 14 enacts "That the same specific name can only be used once in the same genus," but in the small print it is explained that a name which was not valid when first published, because it infringed this law, may become valid by the removal of the earlier species of the same name to a different genus. By this ingenious

contrivance a name may be valid and not valid at one and the same time, according to the classification which different authors may prefer. The name will die when it is born, and then by a stroke of luck come to life, killing some other name which has taken its place. Then perhaps it will be transferred into the genus to which its namesake has been earlier carried, and so perish once more. Can anyone contemplate with patience the possibility of these romantic adventures, when nothing is easier and simpler than to abide by the precept, actually appended to rule 5—once a synonym always a synonym?

Whatever opinion may be held about these various details, this at least should be remembered, that, however great and wise and powerful may be the Association, the Society, the Congress, which promulgates ordinances on this subject of zoological nomenclature, the enactments can never have the irresistibility of Natural Law. They are exposed to some of the weaknesses which affect Statute Law and still more International Law and all codes of honour and social etiquette. They must have public opinion on their side or they will not work. They should be calculated to win respect, to prevail, not by arbitrary power, but by deserved prestige. To those who are the organising forces in this matter I earnestly make this appeal. The regulation of trifles should be laid aside. Allow us to use small initials or capital letters *à discrétion*; allow us to transliterate our Greek and to leave out our aspirates, and in general to show our fancifulness or our want of scholarly education as we choose. In the great essentials make the rules as logical, as simple, as equitable as human wit can devise, and then stand steadfast, secure that the reasonable in all nations will eventually, if not at once, endorse your action.

THOMAS R. R. STEBBING.

IN view of the interest at present taken in this subject, we take the liberty of quoting the following weighty words from the presidential address entitled "The Formulation of the Natural Sciences," delivered to the American Society of Naturalists in Philadelphia by Professor E. D. Cope, and published in the *American Naturalist*, vol. xxx., pp. 101–112, February, 1896. The passage quoted begins on p. 109:— ED. NAT. SCI.

"Nomenclature is like pens, ink, and paper; it is not science, but it is essential to the pursuit of science. It is, of course, for convenience that we use it, but it does not follow from that that every kind of use of it is convenient. It is a rather common form of apology for misuse of it to state that as it is a matter of convenience, it makes no difference how many or how few names we recognise or use. An illustration of this bad method is the practice of subdividing a genus of many species into many genera, simply because it has many species. The author who does this ignores the fact that a genus has a definite value, no matter whether it has one or five hundred species. I do not mean to maintain that the genus or any

other value has an absolute fixity in all cases. They undoubtedly grade into each other at particular places in the system, but these cases must be judged on their own merits. In general there is no such gradation.

"Nomenclature is then orderly because the things named have definite relations which it is the business of taxonomy, and nomenclature its spokesman, to state. Here we have a fixed basis of procedure. In order to reach entire fixity, a rule which decides between rival names for the same thing is in force. This is the natural and rational law of priority. With the exception of some conservative botanists, all naturalists are, so far as I am aware, in the habit of observing this rule. The result of a failure to do so is self-evident. There is, however, some difference of opinion as to what constitutes priority. Some of the aspects of the problem are simple, others more difficult. Thus there is little or no difference of opinion as to the rule that the name of a species is the first binomial which it received. This is not a single date for all species, since some early authors who used trinomials and polynomials occasionally used binomials. A second rule which is found in all the codes, is that a name in order to be a candidate for adoption, must be accompanied by a descriptive diagnosis or a plate. As divisions above species cannot be defined by a plate, a description is essential in every such case.

"It is on the question of description that a certain amount of difference of opinion exists. From the codes of the Association for the Advancement of Science, and of the Zoological Congresses, no difference of opinion can be inferred, but the practice of a number of naturalists both zoologists and palæontologists in America, and palæontologists in Europe, is not in accord with the rule requiring definition of all groups above species. It has always appeared to me remarkable that a rule of such self-evident necessity should not meet with universal adoption. However, the objections to it, such as they are, I will briefly consider. It is alleged that the definitions when first given are more or less imperfect, and have to be subsequently amended, hence it is argued they have no authority. However, the first definitions, if drawn up with reference to the principles enumerated in the first part of this address, need not be imperfect. Also an old-time diagnosis of a division which we have subsequently found it necessary to divide, is not imperfect on that account alone, but it may be, and often is, the definition of a higher group. But you are familiar with all this class of objections and the answers to them, so I will refer only to the positive reasons which have induced the majority of naturalists to adhere to the rule.

"It is self-evident that so soon as we abandon definitions for words, we have left science and have gone into a kind of literature. In pursuing such a course we load ourselves with rubbish, and place ourselves in a position to have more of it placed upon us. The load of necessary names is quite sufficient, and we must have a reason for every one of them, in order to feel that it is necessary to carry it. Next, it is essential that every line of scientific writing should be intelligible. A man should be required to give a sufficient reason for everything that he does in science. Thus much on behalf of clearness and precision. There is another aspect of the case which is ethical. I am aware that some students do not think that ethical considerations should enter into scientific work. To this I answer that I do not know of any field of human labour into which ethical considerations do not necessarily enter. The reasons for sustaining the law of priority are partly ethical, forwe instinctively wish to see

every man credited with his own work, and not some other man. The law of priority in nomenclature goes no further in this direction than the nature of each case requires. Nomenclature may be an index of much meritorious work, or it may represent comparatively little work; but it is to the interest of all of us that it be not used to sustain a false pretence of work that has not been done at all. By insisting on this essential test of honest intentions we retain the taxonomic and phylogenetic work within the circle of a class of men who are competent to it, and cease to hold out rewards to picture-makers and cataloguers.

"Another contention of some of the nomenclators who use systematic names proposed without description, is, that the spelling in which they were first printed must not be corrected if they contain orthographical and typographical errors. That this view should be sustained by men who have not had the advantage of a classical education, might not be surprising, although one would think they would prefer to avoid publicly displaying the fact, and would be willing to travel some distance in order to find some person who could help them in the matter of spelling. But when well educated men support such a doctrine, one feels that they have created out of the law of priority a fetish which they worship with a devotion quite too narrow. The form of our nomenclature being Latin, the rules of Latin orthography and grammar are as incumbent on us to observe, as are the corresponding rules of English grammar in our ordinary speech. This cult, so far as I know, exists only in the United States and among certain members of the American Ornithologists Union. The preservation of names which their authors never defined; of names which their proposers misspelled; of names from the Greek in Greek instead of Latin form; of English hyphens in Latin composition; and of hybrid combinations of Greek and Latin, are objects hardly worth contending for. Some few authors are quite independent of rules in the use of gender terminations, but I notice the A. O. U. requires these to be printed correctly. Apart from this, I notice in the second edition of their check list of North American Birds just issued, only eighteen misspellings out of a total number of 768 specific and subspecific names, and the generic and other names accompanying. These are, of course, not due to ignorance on the part of the members of this body, some of whom are distinguished for scholarship, but because of an extreme view of the law of priority.

"In closing, I wish to utter a plea for euphony and brevity in the construction of names. In some quarters the making of such names is an unknown art. The simple and appropriate names of Linneus and Cuvier can be still duplicated if students would look into the matter. A great number of such names can be devised by the use of significant Greek prefixes attached to substantiatives which may or may not have been often used. Personal names in Greek have much significance, and they are generally short and euphonious. The unappropriated wealth in this direction is so great that there is really no necessity for poverty in this direction. It should be rarely necessary, for instance, to construct generic names by adding prefixes and suffixes of no meaning to a standard generic name already in use.

E. D. COPE."

568.2

V.

The Wing of Archæopteryx.

THAT the wing of *Archæopteryx* possessed five digits has now been urged upon the readers of NATURAL SCIENCE on two separate occasions (1, 2).

The object of this paper is to bring forward a few additional facts which appear fatal to that hypothesis. Although primarily concerned with the wing, it incidentally refers to certain other parts of the skeleton. I write now, not, as formerly (4), from the evidence of photographs, but from a study of the actual fossils in the London and Berlin museums.

With regard to the carpus, it has been contended (2) that only the proximal row of carpals of *Archæopteryx* has been recognised; that this is represented by two bones, one being the radiale, the other the ulnare; and that the former is visible in both fossils, the latter in only

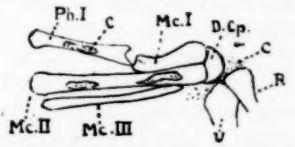

FIG. 1.—CARPUS AND METACARPUS OF THE LEFT WING OF ARCHÆOPTERYX, Dorsal view.

C., Crystalline matter; D.Cp., Distal row of fused carpals; Mc. I, II, III, Metacarpals, I, II, and III; Ph.I, Phalanx I of digit I; R, Radius; U, Ulna.

the London specimen. On this view, the London fossil should present both a radiale and an ulnare. As a matter of fact, however, it has but one bone that can possibly be regarded as a carpal, and this was figured by Owen as the radiale, but is identified by Dr. Hurst (2) as the ulnare. The only visible carpal in the Berlin fossil is certainly *not* a radiale, and it is quite possible that the so-called "ulnare" of the London fossil is not a carpal at all.

The "ulnare" just referred to must be regarded as of prime importance to the hypothesis under discussion. I suggest that it may represent the proximal articular surface of the metacarpals of the right wing, masked by the incrustation of mineral matter.

Only one carpal can be traced in the Berlin fossil, and this has been identified by Dr. Hurst as the "radiale." Fig. 1, which has been

drawn carefully from the actual fossil, would seem to point otherwise. Surely the bone marked Cp. should be regarded, not as the radiale, but as representing more or fewer of the distal carpals fused into one mass, as in living birds. A fine suture line shows that this must have fitted very closely to the metacarpals I and II, if, indeed, it was not actually anchylosed with them. The facts that this bone is strictly confined to the bases of metacarpals I and II, and that metacarpal III was not produced backwards to the level of the bases of I and II, suggest that an additional and separate carpal was present, serving for the articulation of digit III. That there is a stage in the development of the carpus in modern birds (3) precisely agreeing with this permanent condition in *Archæopteryx*, seems to be a point in favour of this interpretation.

Of the five digits which, it is contended, composed the manus of *Archæopteryx*, nos. I, II, and III are supposed to be represented in the Berlin fossil, and nos. IV and V in the London one. The proximal halves (of D. IV, V) are said to have had "ridges which, when the two were fitted together, would have prevented their movement one on the other." I claim to have shown previously, that the bones in question were almost certainly parts of II and III; a further examination suggests that these fragments are encrusted with calcium carbonate, causing these "ridges," which are, in reality, nothing more than striations.

It has been held that the position of these bones "with reference to the feathers of the wing, in spite of the dislocation of other bones, is just that of the large metacarpals in an ordinary bird's wing; and the fact that these feathers are still in their normal position in this wing (the left) justifies the belief that when the animal finally settled down, . . . those feathers were still bound to those metacarpals by ligament." Now, I do not doubt for one moment that if these two bones are metacarpals, they supported those feathers; but to claim for the London fossil that these feathers are still in their normal position strikes one as a stretch of the imagination.

The London fossil has been held by Dr. Hurst to afford the strongest support to his hypothesis. But the stoutness of the two metacarpals may be due to the incrustation of calcite. Furthermore, seeing that the London fossil is, as Dr. Hurst clearly showed, that of a larger bird than the Berlin example—and, therefore, possibly of a different species—there seems to be no reason why the two should, of necessity, agree in detail, or even represent a precisely similar stage of development as regards the skeleton of the wing.

The phalanges of the three digits of the Berlin fossil are stated to have been furnished with tubercles for the insertion of the flexor muscles. This showed that these digits were "used to grasp parts of trees," which they could not have done had they borne long quill-feathers. Though I looked carefully for these tubercles I could only satisfy myself of the existence of anything like a tubercle in the case

of the base of the claw of digit II and the proximal end of the pollex of the left wing.

It is certainly open to question whether the metacarpal bones were "free," as has hitherto been stated; and this is a point which is surely of considerable importance in the present discussion. As to this we have but the evidence of the Berlin fossil. Metacarpals I, II are so closely approximated that only a very faint trace of a suture can be seen. The proximal end of digit III is a little damaged, but it is nevertheless seen to be closely fitted to the base of Mc. II. Distally the II and III metacarpals seem to be slightly separated one from another. I am convinced, however, that this is but an apparent, not a real, separation, and that the two bones are really joined by an osseous bridge, which, being somewhat concave, has become filled up with rock.

If my interpretation of the manus of *Archæopteryx* proves to be correct, then it will, I think, be no longer possible to entertain the hypothesis that these three digits were used for climbing. Thus it will follow that they (or at least the II and III) supported the remiges, so that we shall be able to return once again to our old love the tridactyle avian manus.

I have already urged (4) that if the digits I, II, III were used for climbing, and if the remiges were supported by two additional digits, then the manus of *Archæopteryx* must have undergone an extreme specialisation in two directions, to fit it for two different modes of locomotion—flight and climbing. So far this objection has not been met to my satisfaction.

The fact that the digits of the Berlin fossil appear to lie on the feathers of the wing, and not under them, as we should have expected, has been held as proof that they could not have been concerned in the support of these remiges. This is by no means so serious an objection as it appears. In the London fossil, the author of the above objection assures us, the remiges are still in their *normal* position with regard to the bones which supported them. Now since in both fossils the bones of the digits lie above and not below the level of the remiges, it is hard to see how the same relations can be made to prove both ways.

Professor Dames, I believe, thinks it possible that what is preserved in the Berlin fossil is rather a combination of impressions of the feathers of both dorsal and ventral surfaces of the wing, the feathers themselves having quite disappeared. The remarkably fine impression of the primary remiges, however, would seem to oppose this view. The feathers are so perfectly preserved that even the overlap can be distinctly made out. This, as I suggested, and as Dr. Hurst afterwards maintained, is "distal"—*i.e.*, the outer edge of every remex is free and overlaps the inner edge of the one next in front. This can be well made out in the fossil, where, as photographs first suggested to me, the mud has drifted

under some of these feathers, and so has raised the free or "distal" edges distinctly above the level of that portion of the vane of the feather immediately underneath.

It is significant that, in the Berlin fossil, the bones of the cubitus lie above the level of the remiges, and that the bases of *all* these feathers are completely obliterated. This latter fact suggests that they have suffered disintegration owing to their intimate connection with the decomposing muscles and tendons in which they were imbedded.

Looking at the fossil, one feels tempted to regard a peculiar triangular area between the humerus and cubitus of the left wing as an impression of the patagium.

Dr. Hurst has described "a series of feathers, which may perhaps be classed as rectrices, along the sides of the hinder part of the trunk, . . . these lateral rows of feathers constitute with the tail-feathers a continuous aëroplane, extending forwards as far as the posterior edge of the extended wings." Though I looked most carefully, I entirely failed to see anything approaching such an aëroplane. The tail-feathers I made out to decrease in length from behind forwards, and, at about the fourteenth caudal vertebra, to pass almost insensibly into larger, somewhat backwardly curved, and *decidedly* weaker feathers which cease in the region corresponding with the level of the preaxial border of the femur. Of the tibial *quills* which have been described, I can only say that I saw a mass of feathers which by no stretch of the imagination could I liken to the clearly-defined rectrices and remiges; rather do they resemble a precisely similar series to be found on the tibio-tarsus of some modern birds, *e.g.*, Accipitres.

That the humerus lacked a pectoral crest, and that the bones were *not* pneumatic, are statements that have more than once been made. A reference to the numerous photographs, or better still, to the fossils, shows at once that the pectoral crest was in reality fairly developed. That the long bones were pneumatic cannot be definitely disproved, inasmuch as in both the London and the Berlin fossil, wherever these bones are broken they are seen to be filled with crystal (calcium carbonate), the bone having the appearance of a thin investing sheath just as in fossil bones that are admittedly pneumatic. The existence of the pneumatic foramen has been denied, on the ground that there is no trace of it in either fossil. That there should be any such trace was hardly to be expected, since the crista inferior which lodges this foramen is yet imbedded in the stone.

Whence comes the evidence that the pelvis of *Archæopteryx* was conspicuously unlike that of modern birds in the matter of width? In these latter, the pelvis is supposed to have widened considerably to allow for the backward displacement of the viscera. Since the width of the pelvis is largely determined by that of the sacrum, the little evidence we have, if of any value at all, shows rather that the width of the pelvis of *Archæopteryx* was greater, not less, than in living

birds; inasmuch as, according to Owen, the sacrum was "nearly 1 inch in breadth," while that of a common fowl measured across the widest part is only three-quarters of an inch.

I may conclude by offering a

SUMMARY OF THE CHARACTERS OF *Archæopteryx*.

Reptilian.—Skull: Upper jaw and mandible armed with teeth, apparently set in sockets (Thecodont dentition).

Vertebral column: Centra of vertebræ with flat articular surfaces (Dames *apud* Hurst). Caudal vertebræ numerous, forming

FIG. 2.—ARCHÆOPTERYX LITHOGRAPHICA, RESTORED AFTER THE FOSSIL IN THE BERLIN MUSEUM.

a long lizard-like tail, and each vertebra furnished with a pair of rectrices.

Ribs: The dorsal ribs have been described as wanting uncinate processes; an unsafe conclusion, since these are often absent in the skeletons of existing birds, having been lost in maceration. The cervical ribs appear to have been much more slender than in modern birds, and to have remained moveably articulated throughout life. "Abdominal ribs," resembling rather those of the Crocodilia than of the Chameleonida, appear to have been present.

Manus: Digit III had four phalanges, the last of which was armed with a claw.

The characters just enumerated have passed more or less entirely

out of the life-history of modern birds, but the following yet obtain, either as transitory developmental features or throughout life.

Avian.—Carpus: Probably agreed with that of modern birds; except that the distal mass of fused bones remained distinct throughout life, and that digit III was provided with a separate carpal bone.

Manus: Agrees with that of modern birds, in that it is made up of three digits; also in that digit I had two phalanges and digit II three phalanges, the last phalanx in either case forming a claw. Digit III was also armed with a claw, a feature very rare among existing birds, while it is also peculiar in being made up of four phalanges.

Hind limb: Bird-like throughout; with mesotarsal articulation, distally reduced fibula, three metatarsal bones, elongated and strongly anchylosed one with another—but showing traces of their separate nature in the form of suture lines. Reduction of toes to four, all terminating in a clawed phalanx. Hallux with a short, free, backwardly directed metatarsal (Gadow).

Pelvis: Imperfectly known, but in that its component parts were apparently distinct, resembling that of the Tinamous or the "Ratitæ" among modern birds. So much of the ilium as can be seen is characteristically avian.

Feathers: Their presence on the wings, tail, legs, and in the region of the neck renders it almost certain that feathers clothed the whole body, save the tarso-metatarsus, which was clothed with scales as in existing birds.

The restoration of the entire fossil given in Fig. 2 was undertaken at the suggestion of Professor E. Ray Lankester. My grateful thanks are due to Mr. A. S. Woodward and Mr. C. W. Andrews, of the British Museum, for help and suggestions in examining the London fossil; and to Professor Dames, Dr. Jäkel, and Dr. Böhm for the many and great facilities they afforded me while studying the Berlin fossil, as well as for much kind help and advice.

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Oxford. W. P. PYCRAFT.

SOME NEW BOOKS.

The Whole History of Fishes.

Fishes, Living and Fossil: An Outline of their Forms and Probable Relationships. By Bashford Dean, Ph.D. Pp. xiv., 300, with 344 woodcuts and frontispiece. New York and London: Macmillan & Co., Ltd., 1895. Price 10s. 6d. nett.

For the first time in the history of Ichthyology, students are now provided with an elementary handbook affording a general view of the whole subject. This has not come to displace the still-invaluable "Introduction to the Study of Fishes," published by Dr. Günther nearly sixteen years ago. The latter aims chiefly at a systematic survey of the fishes existing at the present day, with only casual references to their ancestry. The new work for elementary students by Dr. Bashford Dean, of Columbia College, New York, deals rather with the fundamental problems of morphology than with the present-day aspect and distribution of fishes, and, as such, takes a distinct place. The time-honoured perch is deposed from his usual position as *the* fish of all fishes—the standard for comparison with other types of the class. Dr. Dean's view of piscine morphology is much more general, and in conformity with modern research. To describe the skeleton of a perch as that of a typical fish is about as misleading as would be the description of the household of a prince or a millionaire as typical of the ordinary establishment of a civilised man. Dr. Dean recognises this fact and arranges his elementary treatise accordingly.

The brief introductory chapter deals in a novel manner with the adaptation of fishes to life in a watery medium, and then recapitulates a few of the principal results of modern research in reference to their evolution, distribution in time, and classification. The Class Pisces (true fishes with paired limbs and a lower jaw) is divided into the four sub-classes of Elasmobranchii, Holocephali, Dipnoi, and Teleostomi; the last again subdivided into the orders Crossopterygii and Actinopterygii. The table showing "The Distribution of Fishes in Geological Time" is based chiefly upon Zittel's "Handbuch," and needs much revision. More details as to the evolution of structures characteristic of fishes are added in chapter ii., which occupies over forty pages; and these observations are illustrated by numerous instructive diagrams relating to the gills, air-bladder, dermal defences, teeth, fins, and the sense-organs of the lateral line. Most of the exposition will be admitted by all morphologists, and so far as it goes it is very clear; but there is scope for considerable difference of opinion with reference to the section on fins, which is based partly upon the author's own researches, and very largely on those of Thacher, Balfour, Mivart, Dohrn, and Wiedersheim. The concluding remarks on the "pineal eye" are particularly interesting; for Dr. Dean himself has devoted much attention to the subject, and he remarks

"that the pineal structures of the true fishes do not tend to confirm the theory that the epiphysis of the ancestral vertebrates was connected with a median unpaired eye; it would appear, on the other hand, that both in their recent and fossil forms, the epiphysis was connected in its median opening with the innervation of the sensory canals of the head."

The lampreys and their allies, as usual, are treated as a separate class (Marsipobranchii) below the fishes, and form the subject of chapter iii. The Devonian *Palæospondylus* here takes its apparently proper place; while the curious Silurian and Devonian Ostracoderma are also provisionally arranged in the same class. The figures of the latter are not so well chosen as they might have been, but the text is thoroughly up-to-date.

The sub-classes of true fishes are treated in four successive chapters, each arranged on the same plan. First are enumerated the principal structural characters of the members of the sub-class, with at least one figure of the soft parts and one of the skeleton. Next, the more important extinct types are noticed, and then, similarly, the leading surviving types. Finally, the probable affinities and interrelationships of the sub-class are briefly tabulated, in two cases also graphically represented in a diagram.

Among fossil sharks, the Devonian *Cladoselache* occupies the first place as the most primitive, as also the oldest known, genus. As Dr. Dean has proved elsewhere, it represents an otherwise unknown order of Elasmobranchii. The Acanthodians are noticed next, with a restoration of *Acanthodes* which has much too small an orbit. Then follow the Pleuracanthidæ, with a slightly modified copy of Dr. Fritsch's restoration of *Pleuracanthus decheni*, and a very misleading figure by Davis representing an absolutely indeterminable fossil, though labelled "dermal bones of the head roof of *Pleuracanthus*." After a few other remarks on extinct sharks, the brief enumeration of the principal types of existing elasmobranchs begins with *Chlamydoselache* and *Notidanus*, and ends with the rays, which Dr. Dean thinks arose "in early Mesozoic time from the main shark stem, not from Cestracionts, Pristids, Pristiophorids, or even (?) Rhinids." He concludes that, "of all known stems, that of the shark is most nearly ancestral in the line of jaw-bearing vertebrates."

Chapter v., on Chimæroids, is necessarily short, and chiefly remarkable for the beautiful figures of the deep-sea genus *Harriotta*, now first reproduced in a text-book. Fossil remains are also well represented.

The general characters of the Dipnoi are illustrated by a partial modification of W. N. Parker's figure of the soft parts of *Protopterus*, and by a beautiful new drawing of the skeleton of the same genus. There are also new restorations of the Devonian genera *Dipterus* and *Phaneropleuron*. The Devonian Arthrodira, or *Coccosteus*-like fishes, are doubtfully placed at the end of the Dipnoan series, and Dr. Dean's account of the American forms is of importance as being based on personal observation. A series of drawings of the mandibular bone in several of these remarkable fishes is particularly interesting, as also is the new photograph of the skull of *Dinichthys* forming the frontispiece of the book.

The account of the Teleostomi, or "ganoids" and "teleosteans," is compressed within forty pages, and is thus extremely sketchy. All the main features in the development of the group, however, appear to be mentioned; only those readers will be disappointed who seek for a formal arrangement of this sub-class, which comprises the vast

majority of known fishes. The Crossopterygian and Actinopterygian orders are separately treated, but otherwise there is no definite systematic classification. A good series of figures of representative recent fishes are copied chiefly from Günther and Goode; while most of the figures of fossil forms are well-chosen, excluding only those of *Gyroptychius*, *Osteolepis*, *Elonichthys*, *Caturus*, and *Leptolepis*, which are very erroneous, having been drawn by Pander and Agassiz many years ago, when comparatively little was known of the genera they represent.

Chapter viii., on the Development of Fishes, is especially valuable as containing much new matter with good illustrations, and as being the first general synopsis of its kind. The development of the egg and early embryo is successively described in the lamprey, the shark, *Ceratodus*, *Acipenser*, and a Teleostean. This line of research, however, affords much less important information concerning the evolution of fishes than was formerly expected from it. Dr. Dean rightly laments that all deductions from the embryology of fishes are as yet very inconclusive. "The majority of the forms, including some of the most important, are developmentally unknown; yet sufficient is known of the representative members of the groups to show the most perplexing characters. On the one hand, the developmental processes of forms which are regarded by the morphologist as closely akin seem often widely distinct; and, on the other hand, the fishes which should, *à priori*, exhibit an archaic mode of development, actually present complex processes of early growth which can only be interpreted as highly specialised. In fact, there are far greater differences in the developmental plans of the closely-related Ganoid and Teleost, than in those of a Reptile and a Bird; and even among the members of the single group, Teleosts, there are more striking embryological differences than those between Reptiles and Mammals. Adaptive characters have entered so largely into the plan of the development of fishes that they obscure many of the features which might otherwise be made of value for comparison. And until the controversies regarding some of the most fundamental principles in embryology—*e.g.*, the importance of the loss or gain of food yolk—shall be decided, it seems impracticable to use the plan of development as in any strict sense a guide in phylogeny."

The last sixty pages of the volume are devoted to a list of derivations of proper names, a copious bibliography, and a series of illustrated tabular statements of the anatomical characters of the great groups of fishes. These sections bear signs of having been prepared most carefully and laboriously, and form an admirable appendix for purposes of reference. There will be much difference of opinion among specialists as to the value of some of the tables and the judgment pronounced by the author; but we have detected a very small proportion of errors for so bold an enterprise, and students of the lower vertebrata are much indebted to Dr. Dean for an invaluable compendium. To criticise the result in detail would be ungrateful, and we would only allude to two points. Like too many authors of the present generation, Dr. Dean entirely ignores the unrivalled mine of information contained in Owen's "Anatomy of Vertebrates" and his Catalogues published by the Royal College of Surgeons; even Stannius is unaccountably omitted from the list of general works on the anatomy of fishes. Secondly, the drawings ought all to be strict copies of nature, and not provided with embellishments to favour any particular theory, such as happens in the case of fig. 313 (skull of *Ceratodus*) where a non-existent suture is introduced to justify the nomenclature adopted.

In conclusion, we may express the hope that, having made so admirable a beginning, the Columbia University will continue the elementary treatment of the Vertebrata in the same style in four other volumes. A series of handbooks on the plan adopted by Dr. Dean could not fail to attract more students to the subject, and lead to even more rapid progress than now characterises this branch of research.

A. S. W.

Butterflies and Moths.

A Handbook of British Lepidoptera. By Edward Meyrick, B.A., F.Z.S., F.E.S. Pp. vi., 843. London: Macmillan & Co., Ltd., 1895. Price 10s. nett.

Handbuch der Paläarktischen Gross-Schmetterlinge für Forscher und Sammler. By Dr. M. Standfuss. Pp. xii., 392, 8 pls. Jena: Fischer, 1896. Price M. 14.

Die Artbildung und Verwandtschaft bei den Schmetterlingen. II. Theil. Eine systematische Darstellung der Abänderungen, Abarten und Arten der Schwalbenschwanz-ähnlichen Formen der Gattung *Papilio*. Von Dr. G. H. T. Eimer, unter Mitwirkung von Dr. K. Fickert. Text, 8vo. Pp. viii., 153. Tafeln 4, 4to. Jena: G. Fischer, 1895. Price M. 14.

In Mr. Barrett's work on the British Lepidoptera, the second volume of which was noticed in these pages some months since, we have a treatise from the standpoint of a conservative entomologist, who would retain a system of classification which modern research shows us should be abandoned for one more natural. Mr. Meyrick's volume, on the other hand, is the work of an extreme radical. Consternation will surely be felt by many an old lover of the scale-winged insects who may open Mr. Meyrick's book to see what the author has written about the Swallowtail butterfly. Such a reader will naturally turn to the beginning of the systematic portion, but he will find there no account of *Papilio machaon*, or of any other butterfly. The honour of first place in the series is bestowed on the tiger moths (Arctiadæ), and page 325 has to be reached before the "Papilionina" are found, inserted between the Lasiocampidæ and the Pyrales. Aghast at such an innovation, our would-be student, it is to be feared, will refuse to have anything more to do with so revolutionary a book, and will not stop to consider Mr. Meyrick's suggestion that the butterflies are an offshoot of a primitive pyralid stock. Seriously, it might have been better if Mr. Meyrick, in a work intended for British lepidopterists, had shown a little more consideration for the deeply-ingrained conservatism of the class for whose benefit he was writing. Though there is a very high probability that the butterflies and the higher moths have been independently developed from the more primitive Lepidoptera, the first-named group has attained so marked and isolated a standard that its retention at the head of the Lepidoptera would be fully justified. But it is to be hoped that most students will not be hindered by Mr. Meyrick's revolutionary classification from a careful study of a book that will suggest many fruitful ideas and give much matter for thought.

In the seventeen pages of introduction, the structure and life-history of lepidopterous insects, their variation, principles of classification, phylogeny, and nomenclature, are dealt with in as satisfactory a way as such extreme condensation allows. With much reason, Mr. Meyrick defends the use of wing-neuration for systematic purposes, on the ground that it is a non-adaptive character. While similarity in the preparatory stages is doubtless often of value in

indicating relationship, the extreme importance of larval life to the species should lead us to exercise care in relying on it for classificatory characters. Mr. Meyrick often refers to the comparative mobility of the pupal segments in the various groups, recently pointed out by Dr. Chapman; and it seems likely that this character, in conjunction with wing-neuration, will give us approximate indications of the true relationships.

According to Mr. Meyrick's classification, the Lepidoptera are divided into nine main groups, for the names of which the author has, unfortunately, chosen the termination "*-ina*"; there is a general agreement among zoologists that "*-inæ*" shall be a termination distinctive of sub-families. The Caradrinina, which stand at the head of the series, comprise four families—the Arctiadæ, Caradrinidæ (trifid section of the Noctuids), Plusiadæ (quadrifid noctuids and deltoids), and Ocneriadæ (Lymantriidæ of authors). Next comes the Notodont group, including the Geometers (divided into five families), Polyplocidæ (Cymatophoridæ), Sphingidæ, Notodontidæ, and Saturniadæ. Then follow the Lasiocampina, with only three families—Drepanidæ, Endromididæ, and Lasiocampidæ. The butterflies come next, arranged in the usual modern sequence, with the Nymphalidæ at the head and the Hesperiadæ last. Dr. Chapman's recent work on the pupæ of these insects, showing an unexpected relationship between the Pierids and Nymphalids and the isolation of the Lycænids, probably appeared too late for Mr. Meyrick's use. The fifth group of the series is the Pyralina, including the moths usually known by that name (divided into five families), as well as the Pterophoridæ and Orneodidæ (Alucitidæ). Next we find the Psychina comprising the Psychidæ (in which only the three species of *Psyche* are retained, the other genera being removed to the Tineidæ), Zeuzeridæ (without *Cossus*), Zygænidæ, and Heterogeneidæ. The Tortricina form the seventh group, and include the Tortrices (in three families), and the Trypanidæ (*Cossus*). The Tineine group follows, comprising the Ægeriadæ and the Tineæ divided into five families. Mr. Meyrick's arrangement of this most difficult section differs considerably from Stainton's. The degradation of the wing-neuration, owing to the small size of many of the insects, renders that character often unreliable. Probably an approach to a natural system has been secured by comparing the degree of development of the maxillary palps with the number of movable segments in the pupa, and such apparently non-adaptive characters as the roughness or smoothness of head and thighs. The ninth, and last, section is the Micropterygina—(Hepialidæ and Micropterygidæ)—the Jugatæ of Professor Comstock, now generally admitted to represent the primitive lepidopterous stock, and to bridge partially the gap between that order and the Trichoptera or caddis-flies.

The synopses given will be welcome aids to the student seeking to identify specimens. In his definitions of genera, Mr. Meyrick is disposed to be comprehensive. Unfortunately, he has thought it necessary to disinter a multitude of Hübner's generic titles published without description, and to set aside in their favour names in use for the last half-century. The descriptions of the species are concise and clear, but it would have been well to devote a little more space to the varieties which the collector may reasonably be expected to meet. The only illustrations are figures of wing-neuration and profiles of heads and palps, which will be of much use for discriminating genera and directing attention to structure as the basis of classification. A short summary of the geographical distribution of each species in the British Isles and

abroad is given, but so far as Ireland is concerned this information is not altogether trustworthy.

In 1891, Dr. Standfuss published a small "Handbuch für Sammler der europäischen Gross-Schmetterlinge." The work now sent us is an enlarged second edition, but with so much valuable matter added as to make it practically a new book.

The first section of the work deals with the collection of lepidoptera in all stages; the student is told how, when, and where to find and capture eggs, caterpillars, pupæ, and moths. The second section, which occupies by far the greater part of the book, is devoted to the breeding of lepidoptera from both the practical and the theoretical point of view. One of the leading features of this division is the part dealing with crossing and hybrids. A valuable summary is given of the recorded instances of pairing between insects of different species, with notes as to whether any progeny resulted. The author remarks that the hybrid obtained by the crossing of two species differs from the progeny of the same two species if the sexes be reversed. Several of the figures in the very excellent coloured plates illustrate results obtained by the author in a series of experimental crossings with three species of *Saturnia*. Specially interesting are two successful crossings of a male hybrid with the female of one of its parent species. The comparative ease with which these moths interbreed and the fertility of some of their hybrid offspring suggest that the species have not long been separated from each other. From a comparison of the larval and imaginal characters of the three, Dr. Standfuss concludes that *S. spini* is the oldest form and that *S. pavonia* (the only one we have in these islands) is older than *S. pyri*.

Detailed directions are given about the rearing of caterpillars, and warnings as to the various sicknesses to which they are subject. An interesting statistical table shows the relative numbers of the two sexes bred in forty species of butterflies and moths of various families. In every case the males preponderate, the average being nearly 107 males to 100 females. The variation of colour and markings in the Lepidoptera is discussed at some length, and British entomologists will be interested to see references to some of the melanic forms which are so prominent a feature in our own moth-fauna. Dr. Standfuss points out that the Shetland males of *Hepialus humuli* (var. *hethlandica*) must represent the ancestral form of the species—the ordinary white male of this moth being evidently a newer development. He therefore oddly suggests that *H. hethlandica*, Knaggs, should stand as the specific name, *H. humuli*, L., being relegated to varietal rank. Similarly, he holds that the *var. rustica* of *Spilosoma mendicum*, in which the male is white instead of black, represents the ancestral form of that species; while some experiments in crossing the type with the variety show that the latter has the stronger influence on the progeny. It is remarkable that the *var. rustica* should almost entirely replace the typical *S. mendicum* in Ireland, while on the continent it is a south-eastern form, being recorded from Hungary, Roumania, Asia Minor, and Armenia.

A list of species which exhibit seasonal dimorphism is given, with some account of the experiments which Weismann and others have made to elucidate the question. A very complete summary of experiments by the author and others on the influence of heat or cold on the markings of butterflies of the genus *Vanessa* and allies is perhaps the most valuable feature in the whole book. Concluding that the more a species is affected by such experiments the more newly developed it must be, Dr. Standfuss arrives at a phylogeny of

the group which, in the main, agrees with that put forward by Dr. Dixey some years ago, from a minute comparison of the wing-markings. Comparing the Palæarctic species with those of other regions, he would derive *V. atalanta* from its Oriental congeners, and the other species from American forms. On the question of the origin of species, Dr. Standfuss is in agreement with Dr. Eimer's theory of development through external influences.

It is six years since the issue of the first part of Dr. Eimer's work, dealing with the phylogeny of *Papilio podalirius* and its related species. This second instalment traces the presumed development of the Papilios of the *turnus*, *machaon*, and *asterias* groups. The modifications of a typical wing-pattern presented by a related series of butterflies have before offered a tempting field for evolutionary speculation. Here Dr. Eimer brings to our notice a series ranging from the male *P. daunas* with the broad expanse of its creamy-yellow wings crossed by comparatively narrow black bars, through forms (such as our own *P. machaon*) in which the black markings are broader and tend to become joined together, on to species like *P. asterias* and *P. troilus*, whose wings present a wide expanse of black with the cream colour reduced to a few spots. And it is instructive to find at almost the extreme terms of the series the yellow male of *P. turnus* and the dark *var. glauca* of its female.

These butterflies are used by Dr. Eimer as the text for theorising on some of the burning biological problems of to-day. Those who recall the first part of this work, and his book, a year earlier, on the Origin of Species by the inheritance of acquired characters, will expect him to find in the butterflies arguments against Weismann's views. And they will find that, not Weismann only, but the fundamental Darwinian position is attacked. We are told again and again that natural selection is powerless to originate species; it can but preserve species which have already been developed by the action of the physiological laws of growth and modification in organisms, and the influence of environment. Dr. Eimer insists that changes of the markings in butterflies which indicate specific differences cannot be of use to the insects, while they can be initiated by changes in temperature or food-supply. He apparently believes that the living species we have now before us are links in the direct line of a development which may proceed by slow degrees or, exceptionally, by sudden jumps. Curiously, he considers that the *var. glauca* of *P. turnus* was produced in the latter way. As the action of a changed environment on the physiological forces of the organism induces the production of a new variety or species, so constancy of conditions will fix the new form as a species; this principle is called "genepistasis." Then the intercrossing of the new stock with the parent form (if their geographical ranges overlap) is prevented by the action of "Kyesamechany"—that is Romanes' principle of physiological selection, though Dr. Eimer claims to have enunciated the idea twelve years earlier than our lamented Oxford naturalist. As might be expected, Dr. Eimer disbelieves in the mimicry theory, and would explain the facts of resemblance by the action of similar conditions producing similar effects in parallel series. It may be doubted if the argument for natural selection is much weakened by Dr. Eimer's considerations. Though a change in the wing-patterns may of itself be of no use to an insect, yet that change may be the necessary accompaniment of some variation in structure or habit of the greatest possible use.

GEO. H. CARPENTER.

A Real Naturalist.

The Natural History of Aquatic Insects. By Professor L. C. Miall, F.R.S., with illustrations by A. R. Hammond, F.L.S. Pp. xii., 395. London: Macmillan & Co., Ltd., 1895. Price 6s.

In the waste of books dealing with popular natural history, and especially with entomology, Professor Miall's volume is the greenest oasis. In one of the early pages he made us his friend for ever by dissuading the young naturalist from collecting or trying to become a systematist. "The man," says Professor Miall, "who can name many insects can seldom do anything but name them (there are some conspicuous exceptions!)." Of course Professor Miall, like every other scientific man, knows the retort that the systematist will make. The systematist, like the bibliographer, is necessary, and there are faculties that may be trained by the pursuit of either industry. But the faculties trained by schoolboy collectors are as readily trained by the collecting of stamps or buttons, and the collector soon comes to see in his bird or beetle no more than a new specimen, in his case to be prized for its rarity rather than for his knowledge of it. Under Professor Miall's guidance, the schoolboy or other budding naturalist will be led to a pursuit that will train his highest faculties, that will not aid the wholesale extinction of all the rarer creatures, and in which success cannot possibly be accomplished by money.

Professor Miall begins with a general introduction, in which he discusses water as a sphere of life. He supports the view that aquatic insects have taken to the water secondarily. In this he disagrees with the ingenious theory due, if we remember rightly, to Sir John Lubbock, that insects were primitively aquatic, the wings being modifications of tracheal gills. Many of the most generalised insects are terrestrial and aërial, while tracheæ, which are found alike in land and in aquatic forms, would seem to be typically structures of air-breathing terrestrial creatures. Many of the peculiar features described by Professor Miall are most easily explained as adaptations of the tracheæ of terrestrial creatures to aquatic conditions. The author in his introduction, and repeatedly in his book, discusses those curious properties of the surface-film to which he was the first to call attention. The use of the word "film," and still more the relations of the film to the creatures living upon it or under it, suggest that a layer of different tenacity, and perhaps of different composition, forms on the surface; but the author is careful to state that the film is merely a physical condition of the surface of a liquid in contact with air. He shows how most aquatic creatures live either on the upper surface of the film, never really being wetted, or on the under surface not coming in contact with air. The various devices used to take advantage of the resistance of the film, and to break it by force when necessary, are explained in the most interesting and careful way. The greater part of the book deals with the aquatic representatives of the different orders, beginning with beetles and ending with bugs and spring-tails. From these chapters we could quote suggestive and interesting novelties almost without limit, but we prefer to advise our readers to turn to the volume itself.

Plants of Réunion.

Flore de l'Île de la Réunion, avec l'indication des propriétés économiques et industrielles des plantes. Par E. Jacob de Cordemoy. 8vo. Pp. xxvii., 574. Paris: Klincksieck, 1895.

The little island of Réunion or Bourbon, situated to the S.W. of Mauritius, has furnished Mr. Cordemoy with material for a good-

sized flora. In his preface the author discusses the geological history of the island before proceeding to his remarks on its climate and vegetation. The climate is largely determined by the direction of the wind, the mountain chain which follows the longer axis of the island sheltering one side from the S.E. wind, and inducing a dryness which is only interrupted by rains brought by the N.W. cyclone. These differences in climate are reflected in the vegetation. In the south-east it is luxuriant, a bright green carpet covering the ground, which is abundantly watered. The alluvial soil on the coast is extensively cultivated with sugar-cane, maize, manioc, vanilla, tropical fruits, and spices; and screw-pines, bamboos, and palms abound. In the north-west and west, on the contrary, the littoral zone is dry and indigenous species are rare, but the date-palm and other introduced plants will thrive. At a certain altitude, however, the soil is remarkably fertile and plant-life vigorous, thanks to the abundant dews. The damp forests, their trees draped with mosses, ferns, and orchids, characteristic of the soil on the S.E., are absent. As regards the island as a whole, it has, in the writer's opinion, owing to its general outline, differences in altitude, and climate, one of the most varied and interesting floras which could be studied. Commerson seems to have been the first to explore Bourbon botanically. He acted as naturalist to Bougainville's expedition round the world, and afterwards spent five years in Madagascar, Bourbon, and Mauritius, where he died in 1773. He collected a large number of plants and left many notes and sketches. Other well-known botanists who have studied the flora of the island were Du Petit Thouars, Bory de St. Vincent, Gaudichaud, Perrotet, and Boivin. More recently, Charles Frappier, whom Mr. Cordemoy describes as a close and intelligent observer, devoted during thirty years (1853–1883) a large amount of time to the plants of certain districts. Orchids were his special study, and he had undertaken to prepare this order for the flora, but the work was unfortunately interrupted by his death.

Mr. Cordemoy enumerates 1,155 species of flowering plants which are indigenous to or naturalised in the island; there are also 221 ferns and fern-allies, 360 mosses and liverworts, 128 lichens, and 83 algæ. The Fungi have not been worked up. A fair sprinkling of the species are here described for the first time. The usefulness of the descriptions would be greater if some diagnosis had been included; in many cases they occupy a whole page, through which the reader must search for characters of importance. In the part relating to the grasses, the work of Mr. C. J. de Cordemoy, the opposite extreme is reached, for the descriptions of new species are often from their brevity quite useless for purposes of comparison.

Plants of North America.

Synoptical Flora of North America. By Asa Gray, LL.D., and Sereno Watson, Ph.D., continued and edited by B. L. Robinson, Ph.D. Vol. i., part i., fascicle i. Imp. 8vo. Pp. ix., 208. Oct. 10, 1895. Harvard Univ. Price 11s.

"The aim of this comprehensive work is to give a critical treatment of the Flowering Plants of North America, with concise but clear descriptions, synonymy, bibliography, and geographic range of all species and varieties, growing without cultivation on this continent, north of Mexico; also, ordinal, generic, and specific keys to facilitate the identification of plants included.

"Of this extended treatise Dr. Asa Gray published, in 1878 and 1884, two parts including all the Gamopetalous Orders. These parts (re-issued in 1886 by the Smithsonian Institution, Miscel. Coll.,

vol. xxxi.) amount to nearly 1,000 pages, imperial octavo. For some time before his death Professor Gray, continuing this work, was engaged in monographing the earlier Polypetalous Orders. After the death of Professor Gray the preparation of the Synoptical Flora was carried on by Dr. Sereno Watson, Curator of the Gray Herbarium of Harvard University, and, after his death in 1892, by his successor, Dr. B. L. Robinson. Following the original plan of the Flora, the treatment of the Polypetalous Orders will form, when completed, vol. i., part i."

Such is the history of the work of which the first fascicle has lately been issued. This includes the orders of the group Polypetalæ from Ranunculaceæ to the Frankeniaciæ. A second fascicle is, we are told, in advanced preparation; this will continue the group as far as Polygalaceæ. The time which has elapsed since the commencement of the work has necessitated many additions to and some revision of the manuscript of Drs. Gray and Watson. Such alterations have been indicated in the text by an asterisk—rather an unnecessary proceeding, as it adds another to the already sufficiently large number of signs and abbreviations. The mark of many meanings (!) occurs in some of the diagnoses, but there is no indication as to which meaning is here intended. The work has been carefully done and its appearance will be welcomed by all interested in American botany; also by the neo-nomenclaturists as affording a new subject for their criticism, since Dr. Robinson has not always seen fit to follow their rules for the time being.

Another Attack on Darwinism.

NATURE *versus* NATURAL SELECTION. By Charles Clement Coe. Pp. xx., 591. London: Swan Sonnenschein & Co., 1895. Price 10s. 6d.

THESE six hundred pages are agreeably lightened by quotations from lay authors, such as Tennyson, Shakespeare, Canon Kingsley, George Eliot, Goldsmith, St. Paul, and Oliver Wendell Holmes, and we are bound to confess that this happy knack of the author made it easier for us to wade through his barren argument. But we are not in a position to recommend those who are not reviewers to tackle the volume. In a question of theology, or of metaphysic, a gentleman may sit in his library and turn out laborious treatises full of sound scholarship and ripe learning. The interpretation of a clause, or a new light on a subjunctive mood may be elaborated in a study, and, thrown into the world, may prove a wall of separation between nations. But the arguments upon which natural selection depend are of another order. The most studious poring over what others have written, the widest collation, and the most ingenious quotation avail you nothing. You must be a naturalist of the laboratory or of the field even to follow the arguments, which, indeed, are frequently doubtfully expressed in the books. Above all, you must be a naturalist, submitting new facts from the natural world, or new sides of old facts, to gain a hearing. Mr. Coe, we fear, has not grasped the nature of the problems he is dealing with. Naturalists who come across his book will see that he has nothing but words to offer them, and will pass by to some wretched little pamphlet, describing a new nodule on the shell of a snail, or some exact statistics as to the breeding of a louse.

New Serials.

WE have received the first part of the *Ornithologist*, a monthly magazine of ornithology and oology, edited by H. K. Swann, with the

assistance of six other naturalists. It is published by Elliot Stock, London, and H. R. Taylor, New York, at a price of 6d. a part. The first number contains 20 small octavo pages and a half-tone block, illustrating a wheatear's nest built in an old tin can.

There has appeared the first part of *Bulletin de la Laboratoire Biologique de St. Pétersbourg* ("Izvyestiya S. Peterburghicheskiĭ Biologhīcheskiĭ Laboratoriya"), 1896. It is written in Russian and contains papers on the mechanism of the maxillary joint by A. Anīchkīn, and on the mechanism of the movements of the tongue, by P. Lesghaft, who is also the editor.

The Field Columbian Museum, Chicago, commenced author last year with "An Historical and Descriptive Account of the Field Columbian Museum." This has been followed by five separately issued publications, including "Handbook and Catalogue of the Meteorite Collection," by O. C. Farrington; "Contribution to the Flora of Yucatan," by C. F. Millspaugh; "On the Structure and Development of the Vertebral Column of Amia," by O. P. Hay. We have already mentioned W. H. Holmes's "Studies among the Ancient Cities of Yucatan." These beautifully produced pamphlets are not priced, since they "are for gratuitous distribution to libraries and scientific institutions and societies. They may be obtained upon application to the Director."

A somewhat similar publication is the "Mittheilungen aus dem Roemer-Museum, Hildesheim," of which the following parts have already appeared: (1) "Systema Lepidopterorum Hildisiæ," by A. Radcliffe Grote, 4 pp.; (2) "Ein neuer *Actinocamax* aus der Quadratenkreide von Braunschweig," by Professor A. Andreæ, 4 pp. and 1 collotype plate; (3) "Die Apateliden," by A. R. Grote, 18 pp. and 2 collotype plates.

The Société des Americanistes de Paris, which was founded not long ago, has issued, in luxurious style, as large quarto, the first part of its *Bulletin*.

The Société de Spélaeologie, whose foundation we noticed in vol. vi., p. 132, has already issued the three first parts of a publication entitled *Spelunca*. In this are described the explorations of numerous caverns in Europe and other parts of the world, including Australia. The Society also intends to publish Memoirs, one of which is to be by Dr. Filhol on the remains of panthers found in caves.

From Tokyo comes the first number, issued in October, of *Konchū Gaku Zasshi* ("Journal of Insect Science"). It is written in Japanese, except for the explanation of the plate. We commend it to the notice of the editor of the *Zoological Record*.

Literature Received.

Life and Letters of Agassiz, 2 vols., J. Marcou: Macmillan. The Glaciers of the Alps, J. Tyndall: Longmans. Elements of Botany, J. Y. Bergen: Ginn. The Whence and the Whither of Man, J. M. Tyler: Scribner's Sons. Hymenoptera Aculeata of the British Isles, E. Saunders: Reeve.

Notes on Seal and Whale Fishery, Southwell: *Zoologist*. Evolution of Modern Scientific Laboratories, W. H. Welch: *Johns Hopkins Hospital Bulletin*. Upper Palæozoic Rocks of Central Kansas, C. S. Prosser: *Journ. Geol.* Sixteenth Report, Dantzig Museum. Catalogue xvii., Mammifères: Dollfus Huxley and his Work, T. Gill: *Science*. Foreign Book Circular, Scientific Series, no. 63: Williams and Norgate.

Journ. Coll. Science, Tokyo, vol. viii., pt. ii., vol. ix., pt. i. Land and Water, Feb. Proceedings, 1894–95: Royal Physical Society. Nature, Feb. 20, 27, Mar. 5. Knowledge, March. Review of Reviews, March. Literary Digest, Feb. 15, 22, 29. Revue Scientifique, Feb. 22, 29, Mar. 7, 14. Irish Naturalist, March. Revue générale des Sciences, Feb. 15. Feuille des jeunes Naturalistes, March. Naturen, Feb. Nature Notes, March. American Journ. Science, March. Naturæ Novitates, Feb. (3 & 4). Victorian Naturalist, Dec. Science, Feb. 7, Mar. 6. Scottish Geographical Mag., March. The Naturalist, March. Westminster Review, March. Journ. Marine Biol. Ass., vol. iv., no. 2. Botanical Gazette, Feb. Biology Notes, Feb. Scientific African, Feb. Psychological Review, Jan. Oxford Univ. Jr. Scientific Club Journal, nos. 24, 33, 34, 35, 36. Johns Hopkins Univ. Circular, Feb. American Geologist, March. L'Anthropologie, T. vii., no. 1. Timehri, Dec.

OBITUARY.

GIULIO ANDREA PIRONA.

BORN AT DIGNANA NOV. 20, 1822. DIED AT UDINE DEC. 28, 1895.

BOTANIST, geologist, zoologist, and philologist, Professor Pirona was an excellent type of the old school of naturalists. Having received his youthful education in Udine, at the hands of the professor abate Jacopo Pirona, he passed to the University of Padua, where he took his degree in medicine, and served for some years as assistant to the professor. He then became professor of natural history at the Lyceum of Udine, and, while yet young, was elected a member of the Venetian Institute of Science, Literature, and Art, of which he was the president during the last two years, and in the *Atti* of which many of his works were published.

The greater part of Pirona's work was connected with his beloved native district of Friuli. He began as botanist, and his "Synopsis" of the Friuli flora, and "Vocabolario botanico friulano" summarise the labours of many excursions, patient determinations, discoveries of new species, and observations of facts of distribution. His rambles through the country led him naturally to the investigation of its rocks, and his early studies in this direction were benefited by the companionship of the Austrian geologist, Foetterle. Thus originated the "Lettere geognostiche sul Friuli," "Cenni geognostiche" (1861), and a sketch geological map that formed a secure foundation for the more detailed work of subsequent observers. He also elucidated the structure of the Euganean Hills, collaborated in a monograph on the Bellunese Earthquake of 1873, and, in conjunction with Professors Taramelli and Tommasi, studied the earthquake of Tolmezzo in 1889, and the water-supply of Udine. Here, too, may be mentioned his "Monografia delle acque minerali del Veneto" (1862-3). His examination of the rocks led to an important series of papers on their fossil contents, and especially on those curious molluscs known as Hippurites. Both as accurate collector and learned describer he made valuable contributions to the Jurassic and Cretaceous palæontology of Italy. But Pirona's knowledge of molluscs was not confined to their extinct representatives, as we learn from his "Prospetto dei Molluschi terrestri e fluviatili finora raccolti nel Friuli" (1864-5). His claim to the title of philologist is based on the "Vocabolario della Lingua Friulana," which gained a prize from the Government. His scientific attainments, and the share that he took in the municipal life of Udine, gained for Pirona, in 1870, the post of Conservator of the Civic Library and Museum, a post which he held till his death.

Pirona may be summed up in the words contributed by his eminent colleague, Professor Torquato Taramelli, to *La Patria del*

Friuli [1] on the last day of 1895. "From time to time, with admirable clearness and harmonious arrangement, he collected in welcome publications all that concerned the natural history of his Friuli, always enhancing its interest by the purity of his style, and presenting the more certain of the results gained by his own labours or by those of others. He was a naturalist of genius, who for the sake of a flower did not overlook the hard rock below; who did not despise the snail and the butterfly."

CHARLES WACHSMUTH.

Born at Hanover, Sept. 13, 1829. Died at Burlington, Ia., Feb. 7, 1896.

In June of last year Natural Science described the Monograph of the Camerate or vaulted Crinoidea of North America, the result of many years of labour on the part of Charles Wachsmuth and Frank Springer, now on the point of publication by the Museum of Comparative Zoology at Harvard; and that note gave some account of the labours of the chief author, who, everyone will regret to learn, has died before the end has crowned his work. The only son of Christian Wachsmuth, an eminent lawyer and member of the Prussian Parliament, Charles was born and educated for his father's profession in his father's city. Failing health caused him to relinquish law for commerce, and in 1852 he was sent to New York by a Hamburg firm in the interest of German emigration, and in that city he stayed for two years. His duties completed, health caused his migration to Burlington, where he set up in business and made a home for the wife whom he married in 1855. Hitherto he had paid no attention to natural science, but unimproved health forced him to give up business in 1857, and to seek outdoor exercise in the collecting of fossils. Being attracted by the beautiful crinoid remains for which Burlington is famous, and realising that he could do his best work by restricting his energies to some special corner of the field of science, he threw himself with enthusiasm into the study of the Crinoidea. A visit to Agassiz at Cambridge, Mass., and then to Europe, in 1865, inspired him to fresh efforts. He did not, however, venture on independent publication, but assisted W. H. Niles in a paper on the Burlington Limestone (1866), as well as Meek and Worthen in the *Report* of the Illinois Geological Survey. Wachsmuth's collection grew in numbers and historical importance till he was at last induced to part with it for $6,000 to Louis Agassiz, at whose request he accompanied the museum to Cambridge, and spent some time arranging it for exhibition. On his return to Burlington, he set to work to renew his collection, doing this with such success that he was enabled to part with a great portion of it to the Trustees of the British

[1] As quoted in *Rivista ital. di Paleont.*, i., p. 262, where C. Fornasini has a notice from which some of the above facts are taken.

Museum in 1874 for the sum of £80. About this time Mr. and Mrs. Wachsmuth made a prolonged tour through the whole of the Old World, during which he studied the crinoid collection in many foreign museums, and visited the most notable scenes in Italy, Greece, Turkey, Arabia, and Africa. It was Wachsmuth's custom to spend some months every summer on collecting excursions together with his wife; ransacking thus the principal localities of North America, some of which he discovered for himself, and obtaining other specimens by means of collectors, such as the late Charles Beachler, and purchasing yet others with the co-operation of his friend, Mr. Frank Springer, he for a third time gathered together one of the finest collections of crinoids in the world, to receive which he built a special fire-proof museum. Though retiring in his habits, the dead palæontologist was one of the leading citizens of Burlington, a trustee of the free public library, and the vice-president of its board.

Mr. Wachsmuth's health had been declining for three years, and writing to me on January 13 he said, "I doubt if I shall be able again to do scientific work"; his end, therefore, was hardly unexpected. Had he lived but a few months longer, it was hoped that the Geological Society of London would have marked the publication of his Monograph by electing him a Foreign Member. English geologists will regret that their roll of fame must continue to lack so bright an ornament as the name of Charles Wachsmuth. F. A. B.

SEKIYA SEIKEI.

BORN 1855. DIED JANUARY 9, 1896.

THE death of Professor Sekiya leaves a serious gap. His published memoirs are few in number (we can speak only of those written in English), but several of these are of lasting value. Among them may be specially mentioned his two papers on the Japanese earthquakes of 1885, and that on "Earthquake Measurements of Recent Years, especially relating to Vertical Motion." In conjunction with Mr. Kikuchi he wrote an account of the eruption of Bandai-san in 1888; and, with Mr. Omori, a valuable "Comparison of Earthquake Measurements made in a Pit and on the Surface Ground." Beginning his scientific career about the time when accurate seismographs were invented, and living in the country where they were first erected and most frequently put into use, it is only natural that Sekiya's attention should have been attracted to their indications, and some of his most valuable work consists in the unravelling of their records. Outside the ranks of specialists, he is most widely known by his model showing the motion of an earth-particle during an earthquake. By this happy device he has furnished the best illustration we possess of the complicated movements of the ground during a severe shock. In 1886 Mr. Sekiya was appointed the first Professor of Seismology in the Imperial University of Japan.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE following appointments are announced:—Mr. Leonard Hill, to be Lecturer in Physiology at the London Hospital Medical School; Dr. Purcell, to be Assistant at the South African Museum; W. A. Rogers, to be Assistant Geologist to the Geological Commission of Cape Colony; F. E. Willey, of Kew, to be Superintendent of the Botanical Station at Sierra Leone; Professor E. Topsent, of Reims, to the Chair of Zoology at Rennes University; Professor F. Hochstetter, of Vienna, to the Anatomical Chair in Innsbruck University; Dr. Siedentopf, from Bremen, to be Assistant at the Mineralogical Institute of Göttingen University; Dr. L. Buscalioni, of Turin, to be Assistant in the Pflanzenphysiologische Institut at Göttingen; Dr. A. Looss, to be Professor Extraordinarius of Zoology at Leipzig; Dr. Th. Ebert, to be Professor of Palæontology, and Dr. Müller, to be Professor of Regional Geology, in the Prussian Geological Institut; Dr. F. v. Wagner, of Graz, to be Assistant in the Zoological Institute of Giessen University; Dr. R. Bonnet, of Giessen, to be Professor of Anatomy at Greifswald; Dr. W. Kurchinsky, of Kiev, to be Extraordinary Professor of Physiology at Dorpat; C. A. Lindström, to be Professor in Anatomy, Erik Müller, to be Professor in Histology, and J. V. Hultkrantz, to be Prosector in Anatomy, at the Karolinska Institute, in Stockholm; Professor G. B. Grassi, of Catania, to be Professor of Comparative Anatomy at Rome University; Dr. P. Voglino, to be Lecturer in Botany at the University of Turin; W. A. Lucy, of Lake Forest, Ill., to be Professor of Zoology at the North-Western University, Evanston, Ill.; J. M. Coulter, President of Lake Forest University, to be Head Professor of Botany at Chicago University.

MR. H. V. DICKENS, the present Assistant-Registrar of London University, has been appointed Registrar, in succession to Mr. Milman, retired. Mr. Dickens was specially associated with the scientific part of the London University Examinations, and all those who, as candidates and examiners, have come in contact with his excellent arrangements will be pleased to hear of his promotion.

DR. FRIDOLIN V. SANDBERGER, Professor of Mineralogy at the University of Würzburg, who lately celebrated the jubilee of his doctorate, is about to resign from teaching.

PROFESSOR A. N. BEKETOW, of St. Petersburg, on resigning his chair, and Dr. J. Hall, of Albany, have been elected honorary members, and Professor Dareste de la Chavanne, of Paris, Dr. C. D. Walcott, of Washington, and Professor G. Retzius, of Stockholm, have been elected corresponding members of the Royal Academy of Science of St. Petersburg.

THE Oxford Delegacy for the Extension of University Teaching propose to reduce their list of lecturers, on account of its congested state. The movement has been productive of a considerable amount of good, but the present tendency seems to be that the more solid part of the work is being transferred to permanent local institutions, several of which have been created by the movement, and that the lighter part is decreasing in importance.

THE Summer School of Art and Science will be held in Edinburgh during August. Tickets may be procured for either the first or the last fortnight, or for the whole session. Mr. G. F. Scott Elliot is to lecture in the section of geography and geology, and Mr. Elisée Reclus will again be present. Further information may be obtained from the secretary, Mr. T. R. Marr, University Hall.

FOR the first time in its history the House of Commons has passed a resolution in favour of the opening of the National Museums and Art Galleries in London on Sundays, though only "for a limited number of hours after 2 p.m.," and "upon condition that no officer shall be required to attend on more than six days, and that any who have conscientious objections shall be exempt from Sunday duty." The amendment moved by Sir Mark Stewart, that opening should take place on three week-day evenings of each week instead of on Sunday, was rejected by 178 votes to 93. The smallness of the numbers indicates that many members kept away in order to escape the possible censure of their constituencies, for no one exactly seems to know what are the real wishes of the mass of the people on this question. Those who spoke in favour of Mr. Massey-Mainwaring's motion were Mr. Lough, Sir S. Montagu, who claimed to give the views of the poorest classes of London, Sir G. Trevelyan, who put in a word for the professional middle-classes, Mr. Holborn, who came from "sabbatarian Scotland," Mr. Goschen, speaking as a private member, Mr. Burns, who may fairly claim to represent labour, Mr. Lowles, and Sir John Lubbock, who said that "the Trustees of the British Museum were anxious to open that building in accordance with the resolution before the House." Those who spoke against the motion were Colonel Sandys, Colonel Ward, and Lord Warkworth, all of whose objections, based avowedly upon theological grounds are, as *The Times* says, "to be treated with all respect, but have never been a dominant factor in the situation." It does not follow from the passing of this resolution that our London Museums are to be opened this year, next year, or ever. The authorities of the museums and of the Treasury have to be in accord with the resolution and with one another. Mr. Balfour has informed the House that the authorities of the institutions concerned are being consulted; but up to the time of our going to press those authorities had not even received formal intimation of the resolution. A special meeting of the Trustees of the British Museum, convened to consider the question, had, therefore, to be adjourned. The other museums at South Kensington, the Museum of Practical Geology, and the Bethnal Green Museum must receive their orders from the Science and Art Department, and their opening will no doubt resolve itself into a mere question of money. It is greatly to be hoped that the good resolution passed by the House of Commons may not be consigned, from want of the necessary means, to form another paving-stone in a country beyond the scope of this magazine.

THE monthly evening lectures at the Whitechapel Museum are to be given, in April, by Mr. Chalmers Mitchell, on "The Food-Supply of the World"; in May, by Dr. Gregory, on some subject connected with his African expedition; in June, by Dr. A. Keith, on "Whitechapel Ears."

THE Kelvingrove Museum, Glasgow, has purchased from Professor Westerlund 2,000 species and varieties of palæarctic land and fresh-water shells. The Corporation has acquired the Mansion House of Camp Hill, and it is intended to use the main floor as a local exhibition gallery for the southern district of Glasgow. Other galleries are being built at Glasgow, and will doubtless prove of interest to the members of the Museums Association, which will meet in that town during this year, under the presidency of Mr. James Paton, the superintendent of the museum and galleries.

THE American Museum of Natural History sent a collecting expedition last year to the Uintah beds of N.E. Utah, then between the eastern escarpment of the Uintah range and the Green River to the Washakie Beds of S.W. Wyoming. The most important result geologically was the determination that the Brown Park deposit was of much later age than the Uintah.

FROM the sixteenth annual report of the Director of the U.S. Geological Survey, Dr. C. D. Walcott, we learn that certain changes in organisation have been made. The work of the Survey is now distributed under the four heads, Geologic, Topographic, Publication, and Administration, the first of these being again divided

into Geology, Palæontology, Chemistry, Hydrography, and Mineral Resources. During the year ending June 30, 1895, twenty-six field parties were sent out by the Survey. Considerable fresh information concerning the Devonian and Silurian rocks has been obtained by a party investigating the coal regions of West Virginia and Maryland, while it has been found that the Pottsville coal series of Pennsylvania is increased by the addition of lower beds as it passes southwards into West Virginia. The iron-bearing formations in the Lake Superior region have been traced over an obscure area by the help of the magnetic needle. The director himself journeyed through the mining fields of the western States, and increased the local interest in the Survey. A collection of 80,000 specimens of Coal-measure plants was recently presented to the National Museum by Mr. R. D. Lacoe. It contains nearly two-thirds of the original specimens described and figured from the Carboniferous flora of the United States, and is now being arranged by Mr. David White. This museum, says *Science*, has also obtained a number of fossils from the Peace Creek phosphate deposit, including numerous remains of the mammoth, showing it to have had a large average size in Florida, others of *Bison latifrons*, indicating that it was about 9 inches taller than *Bison americanus*, and two molars probably belonging to *Auchenia minimus*. Another skeleton of a huge extinct bison has lately been found in W. Kansas.

THE Field Columbian Museum of Chicago is sending a scientific expedition, under the leadership of Professor Daniel G. Elliot, into South Central Africa, in order to collect rare birds and mammals. Fort Salisbury will be the headquarters of the expedition, which from June to December next intends to explore Mashonaland and the country lying to the north and west thereof.

THE following are some of the lectures to be given at the Royal Institution after Easter:—Professor James Sully, three lectures on Child-Study and Education; Mr. C. Vernon Boys, three lectures on Ripples in Air and on Water; Professor T. G. Bonney, two lectures on the Building and Sculpture of Western Europe, beginning April 16; Dr. Robert Munro, two lectures on Lake Dwellings, beginning May 28. The Friday evening meetings will be resumed on April 17, when a discourse will be given by Mr. G. Lippmann on Colour Photography.

THE following is the list of sectional presidents for this year's meeting of the British Association, to be held at Liverpool, September 16–23, under the presidency of Sir Joseph Lister, the new President of the Royal Society. A, mathematics and physics, Professor J. J. Thomson; B, chemistry, Dr. Ludwig Mond; C, geology, Mr. John Edward Marr; D, zoology, Professor E. B. Poulton; E, geography, Major Leonard Darwin; F, economics, the Rt. Hon. Leonard Courtney, M.P.; G, mechanical science, Sir Charles Douglas Fox; H, anthropology, Mr. Arthur Evans; I, physiology and pathology, Dr. Walter Holbrook Gaskell; K, botany, Dr. D. H. Scott. The evening discourses will be given by Professor Flinders Petrie, and, probably, by Sir Andrew Noble; the lecture to working men by Professor Fleming.

AT the anniversary meeting of the Geological Society, held on February 21, Dr. Henry Hicks was selected president for the current year, in succession to Dr. Henry Woodward. The subject of the retiring president's address was "The Life-History of the Crustacea in later Palæozoic and Neozoic Times."

THE Natural History Society of New Brunswick has the excellent habit of conducting each year a lengthy visit to some locality for the purpose of botanical geological, and zoological investigation. In July of last year the neighbourhood of the Lepreau Basin was thus visited. The basin is a land-locked sheet of water, closed in from the sea by a gravel beach and low cliffs of sandstone. At low tide there are extensive sand-flats on the basin, which are traversed by interrupted creeks and ponds, affording a refuge to the marine animals which the rising tide brings into the basin. Unfortunately, the zoological results were mostly negative;

the basin itself is disappointing, for it is rather barren of any forms except the economic clam. Star-fishes and sea-urchins hardly occur there at all, a fact supposed to be due to the long time the basin is empty at each tide, and the consequent warmth of the water in the pools.

The thirteenth bulletin of this Society, which has just been published, contains an important article on the ichthyology of New Brunswick, together with a catalogue of the marine and fresh-water fishes of the province, by Dr. Philip Cox, who has lately considerably enriched the museum at St. John by a collection of the same. We may also record the fact that a short time ago this society acquired the so-called Gesner Museum, which was the first museum established in New Brunswick, and contained a collection of arms and implements of various nations, together with numerous natural history objects. The founder of this museum, Abraham Gesner, published a volume on New Brunswick, in 1847, in London. The Society's museum is strong in native birds, geology, botany, and marine molluscs, and does credit to the labours of numerous local workers, such as Montague Chamberlain, C. F. Hartt, Professor Fowler, G. W. Hay, and W. F. Ganong. There is, besides, a very good collection illustrating the stone age in New Brunswick. The president of the Society for 1896 is G. H. Hay; the secretary, Geoffrey Stead; and the curators, Thos. Stothart, F. E. Holman, and J. V. Ellis, junr.

IT is interesting to note that the Indiana Academy of Science elected forty-two new members at its annual meeting last December. This Academy has taken Turkey Lake as a station for the exhaustive study of the effects of environment on, and the variation of, its inhabitants. Several reports on the first season's work were laid before the meeting.

IN our last number we alluded to the course of study in marine biology which Mr. Garstang will conduct at Plymouth during the Easter vacation. We learn that Mr. A. H. Church, of Jesus College, Oxford, is prepared to conduct a similar class in marine botany, on which subject he has recently been working at the Plymouth laboratory. The Marine Biological Association has secured the steam yacht "Busy Bee" of Fowey. To complete the purchase a sum of £600 is required, of which £100 has already been promised by Mr. J. P. Thomasson.

THE Committee in charge of the marine station at Millport on the west coast of Scotland has sent us its report for 1895. During that year nine scientific workers carried on their investigations at the laboratory, which was also frequented by some members of the Committee, and by several young students who were engaged in general collecting and mounting. An attempt is being made to increase the museum accommodation, which is specially required, since Dr. David Robertson has agreed to place a great part of his valuable collection at the disposal of the Committee so soon as suitable premises are provided. Only a few hundred pounds is needed, and to raise this the Committee appeals to all interested in promoting the study of natural science in the west of Scotland. The station is well provided with dredges, a small beam-trawl, a Cambridge rocking microtome, and several useful books, and is considered by practical naturalists to be admirable for the study of the fauna and flora of the Clyde sea-area.

DR. F. DAHL, Professor of Zoology in Kiel, is going to Kaiser-Wilhelms-Land, in New Guinea, to investigate its fauna and flora. We also learn from *Natura Novitates* that Dr. H. Schauinsland, Director of the Bremen Museum, is going for ten months to Laysan Island, in the South Sea, to investigate its fauna and flora. For a similar purpose Mr. Zenker is going to Kribi in the interior of the South Cameroons, and Dr. P. Taubert, of Berlin, to the mountains between Brazil and Venezuela.

BY the enterprise of Messrs. Barbier, numerous representatives of various races from Africa, Madagascar, and the western world are to be exhibited in the Champs de Mars during the summer of this year.

CORRESPONDENCE.

Huxley's Eyes.

Permit me to correct Professor T. Jeffery Parker's slip in speaking of Huxley's "grey eyes." Huxley's eyes—that is, the iris thereof—were what is called black—really a very dark brown. It would have been inconsistent with his strongly melanochroic character that his eyes should be grey.

In Huxley's old age a strongly marked *arcus senilis* developed, so as to obscure somewhat the pigment of the iris, and his eyebrows, long as black as ebony, became grey; but that his late assistant should call his eye "grey" is a curious lapse of memory.

E. Ray Lankester.

"Peripatus" and the "Cambridge Natural History."

It appears to me that the difference of opinion between Professor Sedgwick and myself touching the advisability of assigning names to the genera of Peripatidæ that he has characterised is a subject scarcely worthy of serious discussion.

To hold with him that the action I took in the matter "brings no benefit, and only adds unnecessarily to the nomenclature of the group," evinces a surprising lack of appreciation of the significance and utility of nomenclature. I maintain, on the contrary, that the ascription of a number of species so diversified in distribution, development, and anatomy to one genus represents their relationship in a light that is altogether false; and that the benefit gained by adopting a separate generic title for each of the three sections into which the species fall is strictly comparable to that gained by adopting such titles for the Anthropomorphous Apes, or for the "groups" of fresh-water crayfishes. It is, however, reassuring to be told of Professor Sedgwick's horror of adding unnecessarily to the nomenclature of the Peripatidæ, for one certainly would not have looked for any unwillingness on that score in an author who deliberately published two new and wholly unnecessary names for a couple of species which he was perfectly well aware had already been named by his predecessors.

The next statement in Professor Sedgwick's "Apologia" that concerns me runs as follows: "I read Mr. Pocock's paper when it came out, and so far from finding that he added to our knowledge of the genus, it appeared to me that his own knowledge of the facts already established was not up-to-date in at least one important particular." Before committing himself to the first part of this indictment, it is a pity Professor Sedgwick did not study my paper with a little more care. For, since it fell to my lot to point out almost all that we do know of the specific characters of specimens of *Peripatus* from St. Vincent, referable to *P. iuliformis*, Guilding, which had been lost sight of nearly half-a-century, his attempt to make his readers believe that my paper contained no contribution to our knowledge—putting upon it the most charitable construction possible, and acquitting its author of all malicious intent—amounts to nothing more than a confession of carelessness on his part for not finding in my paper this addition, and others that could be pointed out.

The second part of the charge contains Professor Sedgwick's alleged excuse for not mentioning my work. Well, I am quite willing to do penance for any sins I may have committed, if Professor Sedgwick will have the kindness to point out on what substratum of fact my appearance or appearances of ignorance rest. But I would submit that even if an author's knowledge of established facts be demonstrably, and not merely apparently, behind the times in "at least one important particular," that is to my mind no valid excuse for ignoring all the facts established and suggestions put forward in his paper. I do not presume to dispute Professor Sedgwick's

right to think otherwise, if he pleases; all that I wish to point out is that such behaviour is in keeping neither with my experience of what is customary nor with my notions of what is courteous. R. I. POCOCK.

British Museum (Natural History), S. Kensington.

MR. ROMANES' GUINEA-PIGS.

I SHOULD like to call your attention to a statement occurring in the review, in the current number of NATURAL SCIENCE, of Romanes' "Darwin and After Darwin," part II. The statement refers to Romanes' repetition of Brown-Séquard's experiments, and stands thus: "He had not got nearly far enough to show positive or negative evidence of any validity." Now with regard to the sixth class of Brown-Séquard's experiments, Romanes obtained very distinct confirmation. He states that in progeny of guinea-pigs in which dry gangrene of the ears had been produced by injury to the restiform body, a similarly morbid state of the ears was frequently observed, and that this morbid state was *never observed* in animals which had neither been operated upon themselves nor were the progeny of parents which had been operated upon. Further, the letter from Mr. Leonard Hill, quoted in the volume, records the drooping of the left eyelid in *two* young guinea-pigs produced from parents in which a similar droop was caused by division of the left cervical sympathetic. Romanes himself says that he has not been able to furnish any approach to a full corroboration of Brown-Séquard's results; but that is very different from the assertion that the results I have indicated are of no validity. I am inclined to believe that no evidence, however strong, would convince some people of the inheritance of an acquired character, although Weismann himself has had to modify his theory so far as to include the possibility of such inheritance. But the duty of a reviewer is to give a correct impression of the value of a book, and no ordinary reader would imagine from your reviewer's language that Romanes had obtained any definite positive results at all. Most people who read the evidence in question would admit that the results I have mentioned add very distinctly to the strength of the positive proof which has accumulated around Brown-Séquard's experiments, while the most minute criticism and searching tests have failed to shake the validity of positive results confirmed by several experimenters.—Yours faithfully,

College of Surgeons, J. T. CUNNINGHAM.
March 4, 1896.

[After reading Mr. Cunningham's letter, and re-reading my review and the sections of "Darwin and After Darwin" to which he refers, I agree with him that my phrase concerning the validity of experiments was misleading. Every experiment carefully conducted is valid. What I should have said was that the results of the particular experiments referred to proved or disproved nothing. Romanes was repeating Brown-Séquard's experiments, the interpretation of which as instances of inheritance of acquired characters is, to say the least, debatable. To use Romanes' own words, "on the whole" he was unable "to furnish any approach to a full corroboration." The interpretation of the two or three cases in which he was able to corroborate Brown-Séquard remain as doubtful as before.

THE REVIEWER.]

EMBRYOLOGY OF CIRRIPEDES.

I SHOULD like to say a word or two with reference to the notice of my work contained in your February number (p. 84).

It was with some surprise that I read Professor Chun's paper, and noticed the implied criticisms of my own observations on the characters of the appendages of the Cirripedian Nauplii. The larvæ studied by Professor Chun all belong to the latest Nauplius stage, while the Nauplii described in my first paper belong exclusively to the first and second stages. In this communication I showed that the appendages, while agreeing closely in the different species at the same stage, showed differences when the two stages of each species were compared, and in a forth-

coming memoir I shall further show that in the later development the appendages become more and more complex in the successive stages. Professor Chun has apparently omitted to take into consideration the possibility of change at successive stages, and nowhere mentions the circumstances that the larvæ compared were of different ages. As a matter of fact, the differences between his observations and my own nearly completely disappear when the corresponding sixth stages are compared, but not quite, for actual differences do exist in the different species at this stage. I see no reason to depart from the view I maintained, that there is an almost perfect agreement between the different species during the first two stages.

With reference to Dr. Giesbrecht's remarks, which I saw soon after the publication of his beautiful monograph, it seemed to me unnecessary to reply to them, as I thought it would be perfectly apparent to anybody reading the communication by Dr. Loeb and myself that the above-mentioned observer had not read the paper, at any rate with the care that adverse criticism ought to presuppose, for his remarks show a complete misapprehension of our statements.

In the first place, with regard to the statement that the part of a cylindrical vessel filled with water is brightest on the side remote from the source of light, I may say that we took some pains to make it clear that it was not necessarily towards or away from the most brightly-illuminated spots that the Nauplii moved, but *in the direction of the rays of light*, and that a Nauplius in the positively heliotropic condition (*i.e.*, moving towards the source of light) might be made to pass from a more brightly to a less brightly-illuminated region, and *vice versâ*.

In the second place, the rays of light did not always pass through the convex glass front of the vessel, but in many experiments obliquely from above through the water only.

Thirdly, the vessels used were by no means always cylindrical; receptacles of all shapes were employed, including long cylindrical glass troughs with upright sides.

I may lastly add that a later experimenter (Viguier), while severely criticising our views, confirms our observations as to the existence of positive and negative conditions of heliotropism in Cirripede Nauplii. THEODORE GROOM.

Royal Agricultural College, Cirencester.
February 10, 1896.

[Though it is not our business to take up the cudgels for Mr. Groom's opponents, still we suggest that it is inaccurate to say that Professor Chun "nowhere mentions the circumstance that the larvæ compared were of different ages," for, having stated on p. 81 that the Nauplii under his observation had "obviously undergone the last moult before passing into the Cypris-stage," on p. 87 he says, "the first antennæ (*antennulæ*) always, as is well known, uniramous, I find in agreement with the statements of Dohrn (1870, p. 603), and Willemoes Suhm (1875, p. 143), in all observed Nauplii composed of six joints. Buchholz (1869, p. 30) and Groom (1894, p. 179) give the number of their joints in the younger Nauplii of the Lepads and Balani as only four, while Lang (1876, p. 109), found them in the Balanid-Nauplii five-jointed." To the second antennæ Chun assigns an eight-jointed exopod and a four-jointed endopod, nine and three being the corresponding numbers given by Groom, while to the mandibular feet Chun assigns a four-jointed exopod and three-jointed endopod, in contrast with the numbers five and two given by Groom.

It was surely impolitic on Mr. Groom's part to ignore a criticism appearing in such a work as the "Pelagische Copepoden." If we rightly understand the explanation now offered, it is in the nature of a plea of "confession and avoidance," not rebutting Dr. Giesbrecht's accusation, but claiming that, under the circumstances, the error imputed was of no consequence.—ED. NAT. SCI.]

GEOLOGICAL MAPS.

THE colour-printing of Geological Survey Maps alluded to in your February number (p. 142) is certainly a welcome departure; for, on account of the price, the amateur, as distinct from the professional, geologist has hitherto had to colour his own maps from such copies as he could beg or borrow.

But, though colour is all very well for demonstration purposes, and sometimes for use in the field, it has many disadvantages. The darker colours obscure the detail, so as to render it illegible, and in hand-painted maps the colour runs after a wet day in the field. Maps are not always accurate, and it is difficult to alter a coloured map. Again, it may happen that one wishes to colour on a different plan, or for some different object, and yet to be able to have the geological lines visible in the same sheet: this cannot be done in a coloured map. Therefore, it ought to be possible for anyone to purchase plain one-inch maps with the geological boundary lines engraved; and on the inside of such boundary lines the designatory letters should be placed at sufficiently convenient intervals g5. g5. after the plan of contour-lines on the six-inch map. These maps could be produced as cheaply as the present plain maps, and the student could get half-a-dozen of these for the price of a coloured one. From my experience in the field I venture to say that such maps are all the field-worker requires, and that coloured maps are a plague to him. S. S. BUCKMAN.

[While sympathising with Mr. Buckman's request, we may point out that some of the charges he brings, such as illegibility and running of colour, do not apply to the new colour-printed maps.—ED. NAT. SCI.]

A CORRECTION.

IN your note on the University of Minnesota (vol. viii., p. 67), for *die*-testing read *ore*-testing. This misprint is not your fault, but came, as you stated, from *Science*.

Your review of Miss J. E. Tilden's "Bibliography of Algæ" was objectionable. You grumbled because it was incomplete. But it is entitled a *Contribution* to the Bibliography, etc., and was avowedly incomplete. Further instalments will be published, a large one being now almost ready. The intention is finally to correct it up and issue on cards.

I like NATURAL SCIENCE immensely. It is indispensable.—Yours truly,

University of Minnesota, *January* 21, 1896. CONWAY MACMILLAN. Head Professor of Botany.

"A ROWING INDICATOR."

MR. E. C. ATKINSON desires to point out that on page 132, line 39, the words "in cases 3 and 7 in excess or defect" should be "in some cases 1 or 2 in excess or defect." The photograph reproduced in Fig. 1 was kindly taken by P. Elford, Esq., St. John's College, Oxford.

NOTICE.

TO CONTRIBUTORS.—*All communications to be addressed to the* EDITOR *of* NATURAL SCIENCE, *at* 22, ST. ANDREW STREET, HOLBORN CIRCUS, LONDON, E.C. *Correspondence and notes intended for any particular month should be sent in not later than the* 10*th of the preceding month.*

TO THE TRADE.—NATURAL SCIENCE *is published on the* 25*th of each month; all advertisements should be in the Publishers' hands not later than the* 20*th.*

THE "CHALLENGER" NUMBER.—*In reply to enquiries, we remind our readers that, although the* FIRST *edition of this ran out of print immediately, there are still some copies of the* SECOND *edition to be obtained at the usual price*—ONE SHILLING. *No more will now be printed, so orders should be sent at once.*